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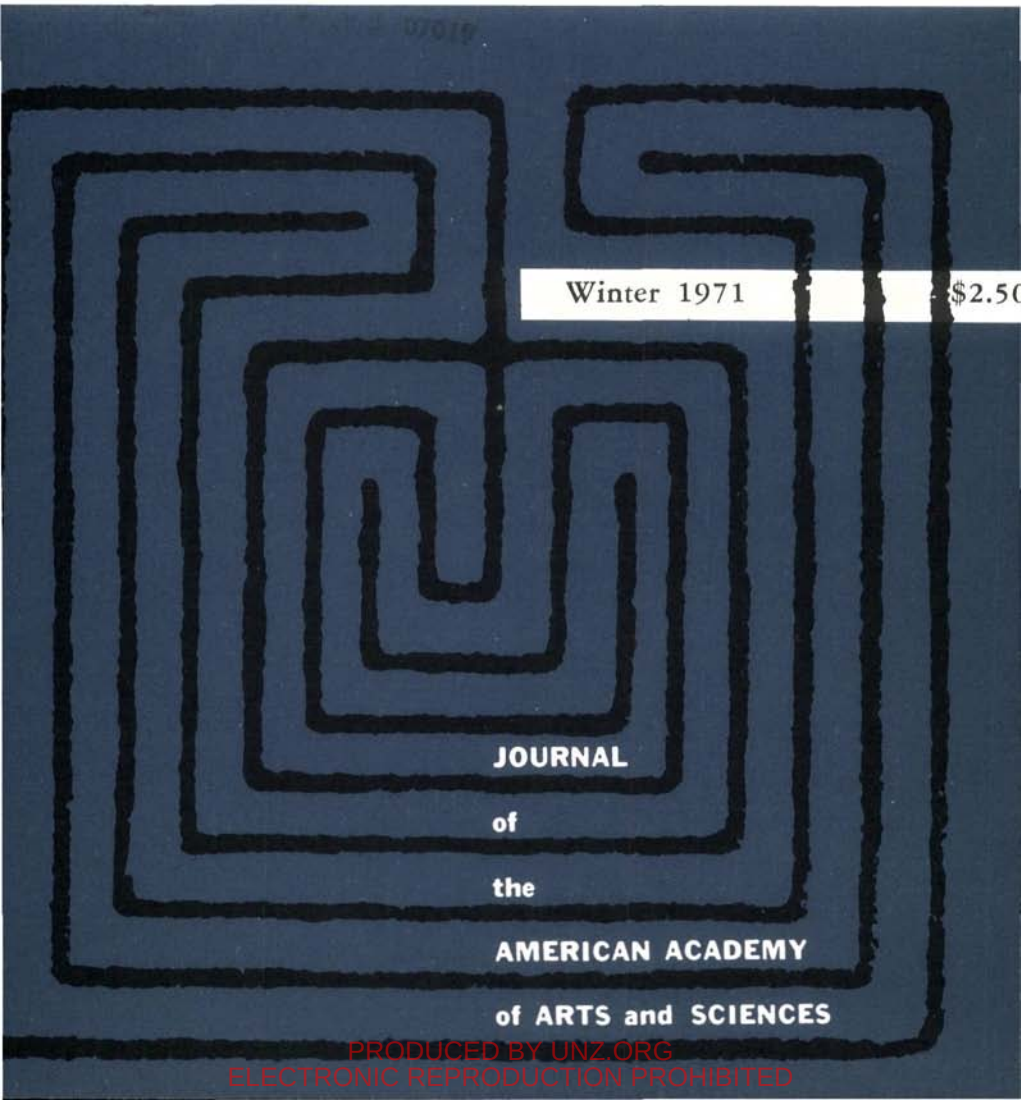

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Historical Studies Today

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DÆDALUS

Journal of the American Academy of Arts and Sciences

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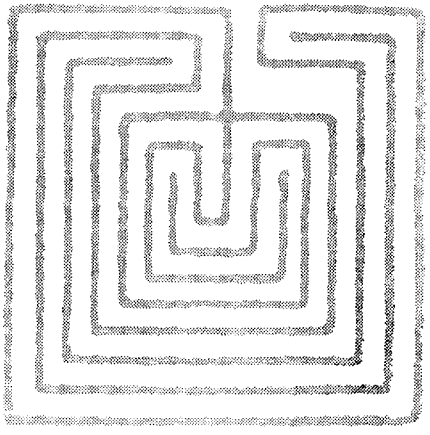

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Preface to the Issue "Historical Studies Today"

In December 1969, in his presidential address at the annual meeting of the American Historical Association, C. Vann Woodward warned his colleagues that the "boom" characteristic of historical studies since the end of the Second World War appeared to be coming to an end. Looking at various indices that suggested a diminished interest in history, Woodward drew attention also to the many evidences of antihistorical bias in contemporary culture; neither the humanities nor the arts, he explained, showed any great partiality for history or historical explanation. The warnings that Woodward eloquently expressed a year ago have been repeated many times since. There is growing acceptance of the fact that a major turning away from historical study is threatened both in the schools and colleges of America, and this is seen to have the most important implications for the character of learning in our society.

It is extraordinary that such a development is occurring at a time when professional historians believe—and their claim is made neither out of immodesty nor out of inadequate regard for earlier historical scholarship—that professional historical investigation is flourishing, and is as robust as it has ever been. While professional historians find their methods and objectives challenged in certain quarters—sometimes explicitly, more often by implication—they are naturally led to wish to explain their work, to indicate how it differs from that of earlier generations.

Those in the profession, excited by what they perceive to be new scholarly opportunities, find it hard to accept the charges of "irrelevance" and "bias" leveled by some who stand outside; yet, there is no possibility of meeting such charges unless an effort is made to describe and explain what historians are in fact doing today. For a profession that is international, involved in wide-ranging studies of very different kinds of phenomena, there is no easy way to render all the major tendencies that may be said to have importance. One can only indicate trends and suggest directions that scholars appear to find congenial.

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Fashions in historical scholarship do not change rapidly. It is significant that many of the analytical schemes now prominent in historical studies owe their origins to tentative efforts first made in the 1920's and 1930's. As Lawrence Stone indicates in his article on prosopography, the historical profession between the wars faced a major crisis, stemming, in his view, from the near exhaustion of the great tradition of Western historical scholarship that established itself in the nineteenth century. Professional historians had generally involved themselves in a close analysis of state archives; their investigations led them to write works dealing largely with institutional, administrative, constitutional, and diplomatic themes. In the years after the First World War there was a movement away from this tradition, a movement that gained great momentum through the development of the so-called "*Annales* school" in France.

Rarely did any group of historians have such a wide-ranging influence as those, inspired by the example and writings of Marc Bloch and Lucien Febvre, who now employed the *Annales* as the principal vehicle for disseminating their "new history." While the influence of the *Annales* school has sometimes been exaggerated—Jacques Le Goff is quick to admit this—its message was unmistakable; it challenged conventional historical writing, and questioned the kinds of documentary evidence favored by most professional historians. Though the *Annales* historians were suspicious of traditional political history, Le Goff suggests that they were in no sense unaware of the importance of politics. Their battle was with historical conventions that permitted the historian to be satisfied to write sprightly narratives, that made history seem an episodic phenomenon in which political activity was a separable part. A new kind of political history needs to be written—Le Goff argues—one that is concerned with "long-term structures" and that involves the study of "the various semeiological systems belonging to the science of politics: vocabulary, rites, behavior, mental attitudes." Politics needs to be made a part of general history, and politics involves nothing less than a study of power. To study power involves analysis of a kind that the nonprofessional historian is rarely capable of making. Le Goff does not suggest that we can do without "a chronology of political events or the biographies of great men," but this is not the work that the professional historian ought principally to address himself to.

A similar concern is reflected in Eric Hobsbawm's considerations on the present status of social history. He sees that one of the

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major tendencies of contemporary historical scholarship is its move from an old-fashioned interest in particular phenomena, defined as belonging to the category of social history, to a concern with the history of society. Social history, in the hands of older historians, was a kind of residual category: it was what was left over after the really important themes—political, constitutional, diplomatic—had been treated. Social history, which once stood at the periphery of the historian's concern, is, according to Hobsbawm, now at the center. This has meant a new respect for certain kinds of data, and a more conscientious effort to secure and interpret materials that would have been ignored in an earlier period. In place of the state documents that once served the historian, wholly new kinds of documentation are now seen to be necessary. Quite new fields have gained prominence: demography and kinship, social classes and social groups, modernization and industrialization, social movements and movements of social protest. Hobsbawm does not imply that such themes did not figure in the earlier work of certain historians; only, that all have recently institutionalized themselves as fields of inquiry, and now can lay claim to organization, methodology, and a system of publications that makes them international scientific enterprises. There are great possibilities for a new history of society, although Professor Hobsbawm does not claim that they are near realization. He writes: "Let me put my cards on the table. I cannot point to any single work which exemplifies the history of society to which we ought, I believe, to aspire."

Lawrence Stone, in his article on prosopography, more commonly described as collective biography, discusses one of the new methods which make a new approach to social history possible. Again, the incentive for such study has not developed overnight; as a form of historical inquiry, it has been building for four decades. While it appears to be concerned principally with social structure and social mobility, it has a political dimension as well; it seeks to explain how class interest may affect political action. While some of the research has dwelled largely on groups that may be called "power elites," there is a second school—"the more statistically-minded mass school," in Stone's words, that is concerned with great movements of popular opinion. Each school is animated by a quite different interest and uses somewhat different methods. While certain kinds of group biography are favored by American scholars, others are preferred by the British, and still others by the French. The purpose of prosopography, however, ought not to be simply

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the development of monographs on individual groups; a larger purpose needs to be envisaged. Professor Stone writes: "It [prosopography] could be a means to bind together constitutional and institutional history on the one hand and personal biography on the other, which are the two oldest and best developed of the historian's crafts, but which have hitherto run along more or less parallel lines . . . it could form the missing connection between political history and social history, which at present are all too often treated in largely watertight compartments, either in different monographs *or in different chapters of a single volume.*"

Investigations in the area of social history have received a strong impetus from the development of a new field which has become fashionable both in Europe and the United States: quantitative history. As François Furet indicates, all sorts of historical themes are subsumed under the title "quantitative history," and not the least of Furet's concerns is to indicate what the subject is in fact about. He sees that quantitative history has certain limitations: it is not equally applicable to all periods of human history and is only really useful for periods where certain kinds of data exist. Thus, for him, "quantitative history presupposes the existence and elaboration of long series of homogeneous and comparable data, and the first problem which presents itself in new terms is that of sources." Serial history, or quantitative history, cannot be narrative; its concern is to resolve certain problems, and it does this most efficiently if it analyzes data that describes a long-term process. Since many of the data that the quantitative historians use were not prepared with any such use in mind, they require an initial manual system of analysis. The task of creating the "archive" is different from any that historians have been traditionally confronted with; the historian is forced to think carefully about the influence that the manner of their construction will have on their quantitative application. Inevitably such analysis involves the use of methods and concepts derived from other disciplines in the social sciences, particularly from economics. The new history provides an opportunity to see change in new dimensions; this, Furet says, can only contribute to a revising of many of the large historical interpretations that have been inherited.

Pierre Goubert's contribution can serve as an example of the vitalizing effect which the new methods of social history have had in a particular field. Local history has for a long time been the work of gifted amateurs and there has been a tendency for the professional historian to disparage or ignore work in this field. Pierre

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Goubert indicates how such highly specialized study, if applying new methods, has provided abundant new evidence, challenging any number of conventional opinions about larger historical phenomena. Thus, for example, the whole notion of the seventeenth-century "crisis" in France has been substantially revised as the result of research primarily concerned with local history. If the story of local history is a "success story," like all such tales it carries certain hazards with it. Professor Goubert is concerned to indicate what certain of those dangers are. The fact that local history appears to be an exercise involving the use of relatively finite materials makes it popular as a thesis subject; but accumulation of individual monographs that have no relation one to the other would, in Goubert's view, be a great waste of human and material resources. Because in Great Britain and the United States the development of local history has developed along lines somewhat different from those pursued in France, Professor Stone has written briefly about both, seeking to supplement the information provided by Goubert.

As part of the new interest in social history, there is also a growing interest in the history of education. This is one of the classic "underdeveloped" fields in historical research. While bibliographical citations in the field are overwhelming, John Talbott points out that too much of it is simply "house history" or a recounting of "the ideas of pedagogical reformers." Major studies of education as a social process are rare. This is the lack that the new historians working in the field are seeking to remedy. Their concern is essentially with the interplay between education and society. The effects of education in perpetuating particular class structures have never been sufficiently studied. If patterns of education contribute to the maintenance of certain forms of social stratification, so, also, patterns of social stratification influence the structure of education. The relations between education and politics is another area which waits to be opened. Even on an issue such as literacy, where it is assumed that a great deal is known, only the most superficial understanding exists. Too much of the earlier history of education, Talbott tells us, was dominated by a narrow Whig interpretation which set out to tell a success story. Suddenly, the realization grows that there is a very different kind of story to tell, and that this calls for new modes of analysis. We may be on the threshhold of a large resurgence of interest in this area. The new histories of education (and of educational institutions) could have a profound influence in thinking about politics, economic development, and social change.

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Talbott argues persuasively that education needs to be seen as a process "deeply entangled in the life of an entire society," and that it should not be made a separate area of study, appropriated by a specialized group of scholars.

Talbott's demand for placing the history of education in a broad context is the more appropriate because the history of education forms a part not only of social but also of intellectual history. The term "intellectual history" is of recent date and is in use almost exclusively in the United States. Study of the subjects with which intellectual history is concerned is traditional however, although, as Felix Gilbert shows, the manner of treatment of these subjects has been changed and refined so that there is justification for the use of a new term. The new development began with the end of the nineteenth century when various intellectual movements—positivistic philosophy, the rise of the natural sciences, Marxian materialism—led to the repudiation of the notion that history was a process controlled by ideas. In Gilbert's words: "What happened in historical scholarship was a narrowing of the area of scholarly concerns: research focused on documentary sources and history became exclusively political history"; in the twentieth century it became necessary for the intellectual historian to work against an opinion that was overtly hostile to his effort. The contribution of Dilthey in providing a new pattern for the biography of an intellectual figure was followed by others who realized that "any intellectual work is the product of its author's mind, but it is also shaped by its function. It is part of an intellectual debate. It answers questions which have previously been raised." By becoming more critical about how the historian analyzes the mind of an individual, the profession became more critical also about describing and defining the intellectual outlook of an entire period. The historian might begin by being concerned about the ideas of a ruling group; he was soon led to interest himself in the ideas of all classes in society. These are new perspectives involving wholly new kinds of materials. Nevertheless, Professor Gilbert does not believe that the role of the intellectual historian is to reconstruct a *Zeitgeist*, or that he will ever be able to do so. Rather, he points out, "it might be more modest to say that the intellectual historian reconstitutes the mind of an individual or of groups at the times when a particular event happened or an advance was achieved. This "modest" task is actually a task of great import: "It is human consciousness which connects the long-range factors and forces and the individual event,

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and it is at this crucial point of the historical process that the intellectual historian does his work."

If political and intellectual history sometimes appear to be on the defensive, Benjamin Schwartz is not inclined to apologize for either. He does not accept the idea that political or intellectual history has lost in significance, or that political history properly studied need lead to episodic history, any more than intellectual history need lead to an exclusive concern with an outstanding individual. Political history for Schwartz involves "conscious activity set within the framework of all the problems, pressures, and constraints" imposed by a particular situation. It is impossible to conceive of politics except as a phenomenon involving conscious intention and conscious activity. Political history need not give undue attention to the acts of small groups or of individuals; it need not be elitist, nor is there any reason for the elitist label to be applied to intellectual history. Using examples from his studies of traditional and contemporary China, Schwartz indicates the numerous questions that the intellectual historian is in a position to ask, and that will involve him in something other than the study of individual intellectual figures. Against Lévi-Strauss's concern with "dissolving man" Schwartz indicates his own preference for an interpretation of history that will aid in "constituting man."

An example of the tasks which the intellectual historian faces and of the manner in which he solves them is given by Robert Darnton. He opens his article by saying: "Historians have always taken what a society writes, publishes, and reads as a guide to its culture, but they have never taken all its books as guidebooks. Instead, they select a few works as representative of the whole and settle down to write intellectual history." Over the last sixty years the weaknesses of that approach have been recognized and the consequences were attempts to develop a sociology of literature indicating who read what at what time. The results of such quantitative studies are frequently doubtful. However, Darnton suggests that they have made an enormous contribution in demonstrating the importance of a literary culture in more than merely literary terms. Darnton explains this view by a discussion of French literary culture in the eighteenth century. As Darnton says, "Books have a social life and an economic value. All the aspects of their existence —literary, social, economic, and even political—came together with the greatest force in the publishing industry of the eighteenth century." He believes that there were basically two kinds of book pro-

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duction and distribution in the eighteenth century: one was legal, the other was clandestine. Both, he argues, are crucial to our understanding of the culture and the politics of the Old Regime. Darnton's essay, by dwelling on both these universes, suggests a wholly new interpretation not only of the Enlightenment, but also of the last years of the *Ancien Régime*.

We have entered a new area not only in studying what individuals and groups thought but also in examining why they thought what they thought. Frank Manuel, in his interpretation of "The Use and Abuse of Psychology in History," accepts that there are historical precedents for a serious historical interest in psychology that go back at least to the time of Vico. However, it was not until the last decades of the nineteenth century that a major breakthrough occurred. It was only with the emergence of Freud that the ground was laid for a more fundamental innovation in the use of psychological concepts in history. Though the Freudian method of analysis was historical, no historian at the time suspected its large potential importance for historical scholarship. The most subtle use of Freud, perhaps, Manuel says, has been made by Erik Erikson. However, he questions whether Erikson's "ideal psychological model of human development in eight stages" is in fact universally applicable. Has Erikson not drawn entirely on twentieth-century psychoanalytic experience? What relevance has such experience to other cultures and periods? Moving from his study of Erikson's work, Manuel considers the efforts by Sartre to develop portraits based on a psychologized Marxist framework. He finds in Sartre's Flaubert "a brilliant synthesis of Marxist and Freudian insights." Manuel accepts that psychological history remains a dubious form of inquiry to many who call themselves professional historians. This, however, cannot obscure the great distance that has been traversed since the early part of the century. Manuel is disposed to cast in his lot with the Freudian "psychologizers." He says: "The new psychologies can open up whole new areas of inquiry by encouraging the historian to ask some direct, perhaps impertinent questions. The restriction of the method to biography, where it has enjoyed at least partial acceptance, should not be a lasting confinement. Historians will have to wrestle anew with symbolic representation on a broad scale."

M. I. Finley illustrates why it is sometimes difficult to bring to bear on the study of history the technical innovations and discoveries of other fields of scholarship. Finley's report on the relation

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between history and archaeology is not encouraging. He suggests that though archaeology has advanced immensely in the last half century, the historian's profit from such archaeological investigation has not been great. What historians require of archaeologists, Finley writes, is "a willingness to devote themselves to precisely formulated historical questions and a far greater consciousness of the value of statistics, for which pencil and paper and elementary numeracy are on the whole sufficient, though a simple computer would do no harm." Archaeologists concerned principally with classification and chronology have been unwilling to respond to such explicit historical demands, and have argued that historical questions must wait for another time. Professor Finley has no sympathy for that plea; he expresses more understanding of the argument that archaeologists are already overworked, and that there is still so much excavation to be done that those engaged in such efforts cannot possibly address themselves to the problems of the historian. Still, there is no question that the present strategies of investigation pursued by archaeologists do not in fact satisfy the author; he writes: "Of what use . . . is the vast outpouring of annual reports on the year's work from which nothing emerges except the occasional isolated fact, often canceled or corrected in the next year's work or the third or the fourth year's? Of what use are more and more excavations when so many older ones have not been fully reported, and no small number have not been published at all? And, finally, of what use is archaeology anyway, apart from the museum pieces that sometimes come out of the debris, if it leads to nothing more than reports?" Mr. Finley blames not only the archaeologists, but also the ancient historians; they are "too often satisfied with impressions, too often nonnumerate and nonquantitative, too often imprecise in the questions they put to themselves, about documentary as about archaeological evidence."

This issue of *Dædalus* could not have been undertaken without the assistance of Professor Felix Gilbert, who has served as Guest Editor. A great debt is owed to him, and also to Professor Lawrence Stone, who assisted us in formulating many of the questions. An early meeting of a small planning group took place at the Institute for Advanced Study at Princeton; we are indebted to the Institute and to its Director, Dr. Carl Kaysen, for providing a congenial setting for profitable and stimulating discussion. Finally, a particular debt is owed the Ford Foundation. A grant from the Foundation

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to the American Academy of Arts and Sciences, to support inquiry in the humanities, has made this issue possible. A meeting in Rome in the summer of 1970 provided an incomparable opportunity for the exchange of views on early drafts of these articles. This is the first of two issues that we propose to publish on "Historical Studies Today."

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JACQUES LE GOFF

Is Politics Still the Backbone of History?

To a historian trained in what, rightly or wrongly, has been called "the *Annales* school," the title of this essay may in itself seem strange. The *Annales* historian was brought up on the idea that political history was obsolete and out of date. Marc Bloch and Lucien Febvre had said so over and over again. They even invoked the great precursors of modern history. Voltaire, in the *Essai sur les Moeurs et l'esprit des nations,* wrote: "For the last fourteen hundred years, the only Gauls, apparently, have been kings, ministers and generals."[1] Jules Michelet wrote to Charles Sainte-Beuve in 1857: "If I had included only political history in my narrative, if I had taken no account of the various other elements of history (religion, law, geography, literature, art, and so forth), my approach would have been quite different. But I needed a great sweeping movement because all these different elements gravitated together to form one whole."[2] Again, referring to his *History of France,* Michelet said: "Here again I can only say I was on my own. Scarcely anything was ever provided but political history, acts of government, a few words about institutions. No one took any account of what accompanies, explains, and is in part the foundation of political history: social, economic and industrial conditions, the state of literature and thought."[3]

At the same time most historians consciously or unconsciously came under the influence of Marxism, whether to follow it, more or less rigidly, or to challenge it, more or less openly. But too hasty a reading of Marx could suggest that he ranged politics among the superstructures of society, and considered political history an epiphenomenon of the history of production relations. There is the well-known passage in the preface to the *Contribution to a Critique of Political Economy:* "The aggregate of production relations con-

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stitutes the economic structure of society, the concrete base on which there rises a legal and political superstructure, and to which certain forms of social consciousness correspond. The mode of production relating to material life determines the pattern of social, political and intellectual life in general."[4] Without necessarily seeing in Marx's attitude to politics, theoretical and practical (*le politique* and *la politique*), the fundamental pessimism ascribed to it by some—usually hostile—commentators,[5] one may still conclude that a conception like the "withering away of the state" is not likely to enhance the prestige of anything to do with politics, political history included.

This might be thought a one-sided view, to be found only in a historian misled by a specifically French tradition and an exaggerated idea of the influence of Marxism. Not at all. Frenchmen have been among political history's stoutest supporters.[6] And Johan Huizinga, neither a Frenchman nor by any means a Marxist, in the course of his work gradually moved away from political history. In *The Task of Cultural History*[7] he accords it no more than a declining ascendancy, based chiefly on the fact that it is both easy and clear. Since Huizinga was not personally attracted by economic and social history, though he noted their "irresistible rise,"[8] he soon turned his main efforts to the establishing of a scientific cultural history.

Economics, society, and culture seem to have monopolized historians' attention for the last half-century. Political history, the insulted and injured, even seems to have been drawn into the epistemological uncertainties arising from the attempt by certain schools of sociology to blur the distinction between practical and theoretical politics. To mention only two leading figures in present-day French sociology, Alain Touraine has recently emphasized the "two-fold weakness" of political analysis in the social sciences,[9] and Edgar Morin points out the "crisis" in politics owing to the invasion of its field from all sides by the techniques and sciences.[10] Will the atomization of politics itself entail a corresponding disintegration of political history, already driven back on uncomfortable positions within its own discipline? To understand the setbacks suffered by political history in the twentieth century, we must analyze the factors that made it flourish before.

Its former ascendancy was doubtless linked to the predominating form taken on, between the fourteenth and twentieth centuries, first by the society of the Ancien Régime and then by the

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society which emerged from the French Revolution. The rise of the monarchical state, of the Prince and his servants, brought to the forefront of the political stage a shadow-show of courtly and government marionettes which bedazzled both historians and people. Aristotelianism in various shapes and forms, especially after the thirteenth century and Aquinas, provided a vocabulary and concepts in which these new realities could be represented. But the triumph of politics and of political history was not immediate. They were adopted rapidly enough in Italy under the stimulus of the rise of the "signorie." But in France, in spite of a step forward under Charles V, the Aristotelian king, who between 1369 and 1374 had Nicole Oresme translate (from a Latin text) Aristotle's *Politics* and *Ethics* and a treatise on economics, it was not until the seventeenth century that the noun *politique* (politics) came into current use, consolidating that of the adjective, which had been established since the sixteenth century. The word *politique* itself probably benefited from the promotion of all the words belonging to the *polis* family. These, together with those deriving from *urbs*—*urbain* (urban), *urbanité* (urbanity), *urbanisme* (town-planning)—between them cover a large part of the semantic field of civilization. It is perhaps through *police* (which did not produce *policé* [organized, civilized] until the nineteenth century) that we arrive at *politesse* (politeness), which appears in the seventeenth century. The realm of *le politique*, *la politique*, and *les politiques* (theoretical politics, practical politics, and politicians) is thus the realm of the elite, and it is from this that political history derived its nobility. It was part of the aristocratic style. Hence the revolutionary aim of Voltaire, to write "instead of the history of kings and courts the history of men." It looked as if philosophical history would drive out political history. But in fact it usually came to terms with it. One example can be seen in the abbé Raynal's *Histoire philosophique et politique des établissements et du commerce des Européens dans les deux Indes.*[11]

The Revolution of 1789, though it ultimately led in the nineteenth century to the transmission of political power to the bourgeoisie, did not destroy the prerogatives of political history. Romanticism made it totter but did not bring it down. Chateaubriand, who could recognize modernity in history as well as in politics and ideology, though he did so only to reject it, was an isolated case.[12] François Guizot, even more than Augustin Thierry, led history further along the path toward history of civilization,[13] but since

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both were primarily concerned with showing the rise of the bourgeoisie, they remained bogged down in political history. But the "conquering middle classes" not only annexed political history in all its glory—they also took as much delight as their predecessors in a historical model which was monarchical and aristocratic: a typical example of the cultural time-lag which makes a parvenu class affect traditional tastes. Michelet is a solitary peak.

To take the case of France alone, not until the beginning of the twentieth century did political history first withdraw and then succumb before the blows of a new kind of history backed up by the new social sciences—geography, and especially economics and sociology. Vidal de la Blache, François Simiaud, and Emile Durkheim were, whether they realized it or not, the godparents of this new history. Its parents were Henri Berr with the *Revue de synthèse historique* (1901), and even more decisively Marc Bloch and Lucien Febvre with the *Annales d'histoire économique et sociale*.

Raymond Aron has shown in his essay on Thucydides how closely political history is linked to narrative and event.[14] The *Annales* school loathed the trio formed by political history, narrative history, and chronicle or episodic (*événementielle*) history. All this, for them, was mere pseudohistory, history on the cheap, a superficial affair which preferred the shadow to the substance. What had to be put in its place was history in depth—an economic, social, and mental history. In the greatest book produced by the *Annales* school, Fernand Braudel's *La Méditerranée et le monde méditerranéen à l'époque de Philippe II* (1959), political history is relegated to part III, which far from being the culmination of the work is more like the bits and pieces left over. Once the backbone of history, political history has sunk to being no more than an atrophied appendix: the parson's nose of history.

But political history was gradually to return in force by borrowing the methods, spirit, and theoretical approach of the very social sciences which had pushed it into the background. I shall try to sketch this recent comeback by taking medieval history as an example.[15] Sociology and anthropology's first and chief contribution to political history was to establish as its central concept and aim the notion of "power" and the facts relating to power. As Raymond Aron has observed, this notion and these facts apply to all societies and all civilizations: "The problem of Power is eternal, whether the earth is worked with a pick or with a bulldozer."[16]

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It should be noted in this connection that analyses made by political historians in terms of "power" go beyond those in terms of "state" and "nation," whether these are traditional studies or attempts to approach the question from a new angle.[17] It is also worth remembering that Marxism-Leninism, which has been accused of not showing enough interest in political history and theory, has for a long time only concerned itself in this field at the level of state and nation.[18] Lastly, while the word politics suggested the idea of surface and the superficial, the word power evokes center and depth. Surface history having lost its charm, political history becomes history in depth by becoming the history of power. This verbal rehabilitation corresponds to a mental evolution foreshadowed by Marc Bloch, who wrote shortly before his death: "There is a lot to be said about this word 'political.' Why should it always be taken as synonymous with superficial? Is not a history which is centered, as it may quite legitimately be, on the evolution of modes of government and on the fate of the governed, bound to try to understand from the inside the facts it has chosen as the subject of its study?"[19]

The history of political depths started off, however, from the outside, with the signs and symbols of power, as in the work of P. E. Schramm. In a number of studies culminating in the great synthesis *Herrschaftzeichen und Staatssymbolik*,[20] he has shown that the objects which were the characteristic signs of possessors of power in the Middle Ages—crown, throne, orb, scepter, *main de justice*, and so forth—are not to be studied just in themselves. They need to be restored to the context of attitudes and ceremonies of which they formed part, and above all to be seen in the light of the political symbolism from which they derive their true significance.[21]

This symbolism was deeply rooted in a religious semeiology which made the political sphere a province of the religious. Among all the signs and insignia, one in particular lent itself to extensive development, with regard both to politico-religious symbolism and to the institutions in which that symbolism was historically embodied. The whole panorama of medieval politics, linked on one side with the hereditary kingships of antiquity and on the other with the relics of monarchy which have survived into modern times, radiated out from the crown. The symbolic field ranged from the material object itself through the coronation rites to the actual kingdom on one hand and the abstract idea of monarchy on the other. A collection of studies on this political panorama at

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the end of the Middle Ages is to be found in *Corona Regni: Studien über die Krone als Symbol des Staates in späten Mittelalter.*[22]

Quite recently Georges Duby recalled the multiple symbolism of the medieval crown in connection with the crown of thorns which St. Louis placed in the Sainte-Chapelle in Paris.[23] The reference immediately presents a problem of method. Is this appeal to "political" objects not perhaps due to the nature of the period in question, and to the fact that in the early Middle Ages texts are comparatively rare? Is this not, then, an ad hoc method rather than a really new and generally applicable way of approaching the problem?

Curiously enough, the historians most interested in these aspects of medieval political symbolism seem to accept such objections and to minimize the importance of their own approach. Thus P. E. Schramm writes: "The investigation of the insignia of power must be supplemented by investigation of the symbolism of power in general. This means that historical research, which first had to rely on chronicles, then became more precise through the use of documents, letters, deeds, and so on, still has a very long way to go in systematic development. There are more objects and evidence available than expected, and an adequate critical method has also been evolved. So the already existing picture may be filled out and enriched. For the insignia used by the ruler tell more about his expectations and claims, and tell it more definitely, than other available evidence. *This applies especially to those centuries for which written sources are very limited.*"[24]

Similarly Robert Folz, who thinks he discerns through different kinds of documentation different realities, writes: "Administrative documents, figurative representations, liturgical rites, external signs such as vestments and emblems—all these, together with a few narrative texts, are our essential sources of information for the first part of the Middle Ages, when symbol clearly predominated over theory as the expression of political form. It is only from the twelfth century on, with the revival of legal studies, that argumentation and controversy start providing an increasingly large part of our documentation."[25]

But the new political history, like all other branches of history, must abandon the old prejudice that only *faute de mieux*, that is, in the absence of texts, must it turn to nonwritten documentation. History has to use all the evidence it can get, taking from every

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kind its own particular contribution and establishing a hierarchy among them all in terms not of the historian's own predilections but of the system of values of the period concerned. This, needless to say, does not prevent him from going on to treat data from the past according to the standards of modern science, and with the help of all its equipment. Every period has a political ceremonial the significance of which it is the historian's job to discern; and this significance constitutes one of the most important aspects of political history. An outstanding result of the recent orientation of political history toward symbolism and ritual has been a revaluation of the significance of kingship within the political system of feudalism. Before, the general opinion had been that monarchy as an institution and the feudal system were antithetical, and that it was out of the decay of feudalism that monarchical power, en route to absolutism, arose at the end of the Middle Ages. According to this view Charlemagne, by his policy of awarding fiefs, which tended to become hereditary estates, as rewards for public service, unconsciously brought into being the force which was to destroy the public authority he himself had tried to recreate, and which was to subdue the royal power that he, by adding to it the dignity of the imperial crown, thought to have made invulnerable. This explanation is now recognized as false in both its terms. It arose from an inability to go beyond the hollow prestige of the state to the study of power itself. But in the new context, with anachronistic concepts of the state abandoned, medieval kingship, particularly that of the Carolingian period, regained its full meaning, and the feudal king was seen to derive his power not despite but within the feudal system.[26]

It was through the methods of comparative history, borrowed from anthropology and the history of religion, that medieval kingship came to have this new significance and that medieval political history was transformed. Various joint publications set the seal on this change. True, the Middle Ages in the West occupied only a small part of the deliberations of the Thirteenth International Conference on the History of Religion in Rome in 1955, the central theme of which was "The King-God and the Sacred Nature of Kingship."[27] This is true also of the volume presented shortly afterwards to Raffaelle Pettazzoni: *The Sacral Kingship—La Regalità Sacra*.[28] But a few years later the *Arbeitskreis fur mittelalterliche Geschichte*, led by Theodor Mayer in Constance, devoted a volume of its *Vörtrage und Forschungen* to medieval kingship. Meanwhile

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the work of Ernst H. Kantorowicz was growing up parallel to that of Schramm. Kantorowicz, after depicting the greatest sovereign of the Middle Ages, Frederick II,[29] went on to study medieval worship of rulers through liturgical acclamation.[30] His research culminated in the masterpiece *The King's Two Bodies* (1957), which restored to its general historical background the conception of political theology which is an essential key to the understanding of the Middle Ages.[31]

Such were the results, in medieval history, of the trail blazed by Sir James George Frazer, whose research into the magical origins of kingship[32] probably stimulated the historians' own researches into medieval kingship, whether or not they were conscious of the fact or prepared to admit it. One historian at least made no secret of his debt, though he did not always agree with Frazer and pursued his own studies according to specifically historical methods—Marc Bloch. His pioneer work, *Les rois thaumaturges,* published in 1924, is still in the forefront of its field. Bloch is not content merely with describing manifestations of the healing power ascribed to the kings of France and England, or with tracing its history from its emergence to its disappearance and explaining the theories behind it. He also tries to go back to the springs of the collective psychology involved, studies its "popularity" (book II, chapter I), and attempts to explain "*how* people believed in the royal miracle" (pp. 420-430). In short, he draws up a study model of "political mental attitudes," which he puts forward simply as a special case—unique only in terms of its subject—of general forms of mental attitude and sensibility. But in the vitally important though as yet unexplored area of the history of mental attitudes, as far as mental attitudes relating to politics are concerned almost everything still remains to be done. Naturally there can be no question of applying to the men of the Middle Ages the opinion poll methods which can contribute to the study of modern political attitudes. But for the history of public opinion in the Middle Ages, as for other questions, a problematic, theoretical approach to the problem can be established.[33]

It may be noted at this point that political history and the sciences which have influenced its recent evolution have sometimes alternated in using one another as stepping stones. Thus, as we have seen, medieval political history was transformed and enriched by adopting methods borrowed from anthropology: new light was thrown on medieval kingship by studies in archaic or primitive kingship. Medieval political history thus seemed to leave

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the surface ripples of episodic history for the deep diachronic strata of proto- or para-historical societies.

Meanwhile, conversely, anthropology opened itself to historical approaches, and scholars and researchers increasingly turned to political anthropology.[34] This method recognized, in societies "which have no history," structures of disequilibrium and conflict, and established the theoretical preliminaries necessary for providing them with a political history. In so doing it brought out the fact that dynamic social history is not incompatible with an anthropological view of societies and civilizations. Political history did not necessarily lose its dynamism by turning toward anthropology—it might even rediscover in it the schemas, Marxist or otherwise, of the class struggle.[35] Moreover, the vocabulary and mental attitudes of the Middle Ages lend themselves to the formulation of structures and social behavior in terms which are partly political. The upper strata of society are often designated in medieval texts by the term *potentes*, the powerful, generally in contrast to the *pauperes* or poor; sometimes they are referred to as the *superiores*, as opposed to *inferiores*.[36]

This corroborates researches in various sectors of medieval history which have identified in the basic phenomena a *political* dimension, in the sense of a relationship to *power*. The most striking example is the theory according to which, at various dates but usually around the year A.D. 1000, the *seigneuries foncières*, based on dues levied on land and its economic exploitation, gave place more and more to seigneuries based on the lords' powers of leadership, organization, and justice: these were known as *seigneuries banales*, from *ban*, the name for this kind of feudal power. Thus the whole feudal structure right down to its foundations takes on a coloring which is ultimately political.[37] This conception of feudalism, which does not exclude a final explanation in terms of production relations, has the virtue of emphasizing the importance of political factors, in the widest sense of the term, in the functioning of the feudal system, and the weight exerted by political forms in the dynamics of history.

The political aspect crops up again in cultural history. Education is a power and an instrument of power. The gulf between litterati and illitterati which lay so long between clerics and laymen, whether the latter were otherwise powerful or not, shows social cleavages arising out of demarcations between possession and non-possession of different forms of power, between participation and

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nonparticipation in these forms. For example, in the case of members of the university a dual relationship with power begins to emerge in the thirteenth century. On the one hand the world of the university tends to form itself into its own kind of supreme power, alongside the power of the church and the king—*studium,* alongside *sacerdotium* and *regnum.*[38] All those who enjoy the privileges of *studium* participate in its power. At the same time, the result—if not the goal—of university studies and distinctions becomes the attainment of some post or function in lay or ecclesiastical society which leads to participation in the other kinds of power.[39] If, in spite of the difficulties involved, a prosopography of university students and masters in the Middle Ages could be worked out,[40] it would be possible to measure the impact of the university group on the organization of medieval society, and there is no doubt that it would emerge in the character and role of a "power elite."

New light could probably also be shed on medieval political history by studying the application, in the Middle Ages, of the Dumézil schema for Indo-European societies. We know the tripartite schema was in use from the end of the ninth century, and that in the eleventh it took on the stereotyped form of *oratores, bellatores, laboratores.* If we knew how and why these ideas reappeared in the Middle Ages, and what was their mental, intellectual, and political effectiveness, we should probably be able to trace more clearly the different aspects of medieval power, their structures, relationships, and functioning. In my view, we should find that this schema was one of the ideological bases of royal power, the latter subsuming and acting as arbiter between the three functions.[41]

Even the realm of art would be illuminated by the application of political analysis in the broad sense. It is not merely a question of measuring the influence of patronage on the form, content, and evolution of art.[42] It is above all a matter of analyzing how the power of works of art is ordered in relation to power in general. It seems to me that Erwin Panofsky took a first step in this direction when he connected the Gothic style, through the multivalent notion of "order" (and hierarchy), with scholastic method; and then related both to a sociopolitical order embodied in the Ile de France around A.D. 1200 by the Capetian monarchy.[43]

Above all Pierre Francastel, in *Peinture et société: naissance et destruction d'un espace plastique, de la Renaissance au cubisme*

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(1951), has shown not only that politicians—the Medicis in Florence, the patriciate in Venice—understood "the power of figurative images of space" and made them instruments of their policy[44] ("Botticelli's Venus is a policy made explicit"), but also that the new representation of space in terms of perspective is linked to a mental revolution, to a mythical thought governed by "the social and economic policy of giving."

In the realm of religious history one can cite as an example the underlying links between heretical movements and political parties, a subject in which research has scarcely begun.[45] Similarly, in a context relating at once to geography, sociology, and culture, one could point to many modern studies in urban sociology[46] which show the towns, and especially the town-planning, of the Middle Ages, as both an expression and a vehicle of urban power and its possessors. W. Braunfels has made an initial study of this kind for the cities of Tuscany.[47]

Finally, one can see coming into being—and it would be a good thing if it were hierarchized even farther—a differential political history functioning at various levels, according to what Fernand Braudel has called "the times of history."[48] In the short term there is traditional political history: narrative, episodic, full of movement, but anxious to pave the way for a deeper approach. Every so often it proposes quantitative evaluations; it initiates social analyses; it accumulates evidence for a future study of mental attitudes. In the longer term, to be established according to the model for long-term movements proposed by François Simiand, there will be a history of the phases or trends of political history, in which no doubt, as Braudel hopes, social history in the broad sense will still predominate—political history with a sociological emphasis. In between these two types of history, as in economic history, there would be an area of common ground specially devoted to studying the relationships between secular political trends on the one hand and, on the other, short-term movements and episodic highs and lows: a history of crises, in which structures and their dynamics are revealed in their nakedness by the turmoil of events. Lastly there comes a political history which would be almost immobile if it were not linked, as political anthropology has shown it to be, to the essentially conflictual and therefore dynamic structure of societies —a political history of really long-term structures, comprising both the valid, living part of geopolitics and also analysis based on anthropological models. At every one of all these levels, particular

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attention would be paid to the study of the various semeiological systems belonging to the science of politics: vocabulary, rites, behavior, mental attitudes.

Although one may, as I did at the beginning of this essay, speak of a certain crisis at present in political history, it is also true that political aspects and approaches are of increasing importance in the human sciences. Not only does the new science of politicology now contribute its concepts, vocabulary, and methods, but geopolitics too, still alive and kicking though somewhat discredited, political sociology, and, as we have seen, political anthropology, all give political history nourishment and support.

I have described it as a new political history, different from the old—dedicated to structures, social analysis, semeiology and the study of power. This is certainly an overoptimistic picture. It is true I have every so often recalled that much or all still remained to be done in certain directions. But the fact is that the new political history I have tried to sketch is as yet a dream rather than a reality.

Worse, the old political history is still a corpse that has to be made to lie down. True, a grammar of political history is and will always remain not only useful but necessary. We cannot do without a chronology of political events or the biographies of great men. In spite of the progress of democracy, political history will always be, not only but also, the history of great men. And now, thanks precisely to politicology and sociology, we know better than before what an event is, and what constitutes the conditioning of a great man.

But there is still a danger that political history in the vulgarized form in which it appears in countless popularizing books and magazines may once again invade the real science of history. There is a danger that historians of economics and culture may be satisfied with producing a political history of economics or culture, that is, a history of economic or cultural politics. The reason for this is still the same as it was when Lucien Febvre first inveighed against pseudohistory as a kind of history which "makes few demands. Very few. Too few."[49] And pseudohistory still seems ready to be happy with half-measures. While it agrees to lift itself up from the level of events and great men (from which one can always sneak in again by the back door to political history) to the level of institutions and environments, it will still stick if it can with outmoded conceptions of government or state. It puts up a poor

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show against strict juridical conceptions: law, the hope of mankind, is the historian's nightmare. It likes to dabble in the history of ideas and political thought—but often both ideas and politics are superficial. With the best will in the world it remains the most fragile form of history, and the one most likely to succumb to all the old temptations.

I conclude with a fact perhaps worth restating. However much political history may be renewed and regenerated by the other human sciences, it cannot aspire to autonomy. To divide a single branch of learning into separate compartments is more inadmissible than ever in the age of pluridisciplinarity. The comment of Lucien Febvre, cofounder of the *Annales d'Histoire Economique et Sociale,* is truer now than ever: "There is no such thing as economic or social history. There is just history."[50] But it is still true that the models of the new general history must accord the dimension of politics the same place as is occupied in society by the phenomenon of power, which is the epistemological incarnation of politics in the present. To pass from the age of anatomy to that of the atom, political history is no longer the backbone of history but its nucleus.

This article was translated from the French by Barbara Bray.

References

1. Cited by Marc Bloch, *Apologie pour l'histoire, ou métier d'historien,* 4th ed. (Paris: Colin, 1961), p. 90.

2. *Ibid.*, p. 78.

3. Cited by P. Wolff, "L'étude des économies et des sociétés avant l'ère statistique," in C. Samaran, ed., *L'histoire et ses méthodes,* Encyclopédie de la Pléiade, 11 (Paris: Gallimard, 1961), p. 847.

4. For example, on page 4 of the introduction to the interesting volume on *Le féodalisme,* a special number of *Recherches internationales à la lumière du marxisme,* no. 37 (June 1963), the editors write "We have primarily included studies dealing with economic and social relations, with a few excursions into the field of institutional or cultural superstructures."

5. For example, the particularly hostile account given by J. Freund in *L'essence du politique* (Paris: Éditions Sirey, 1965), pp. 645ff. According to Freund, political alienation for Marx is alienation supreme, absolute, and irretrievable.

6. Charles Seignobos wrote in 1924, in the preface to his *Histoire politique*

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de l'Europe contemporaine, that we must "recognize the degree to which the superficial phenomena of political life dominate the fundamental phenomena of economic, intellectual, and social life" (cited by Wolff, "L'étude des économies," p. 850).

7. "The problems of political history are as a rule immediately obvious." Johan Huizinga, *The Task of Cultural History,* written in 1926, published in Dutch in 1929 and in English translation in *Men and Ideas* (New York: Meridian Books, 1959), p. 27. Again: "The historical forms of political life are already to be found in life itself. Political history brings its own forms: a state institution, a peace treaty, a war, a dynasty, the state itself. In this fact, which is inseparable from the paramount importance of those forms themselves, lies the fundamental character of political history. It continues to enjoy a certain primacy because it is so much the morphology of society par excellence." *Ibid.*, pp. 58-59.

8. For example, in "The Political and Military Significance of Chivalric Ideas in the Late Middle Ages," first published in French in *Revue d'histoire diplomatique,* 35 (1921), 126-138, and translated into English in *Men and Ideas.* Huizinga writes (pp. 196-197): "The medievalists of our day are hardly favorable to chivalry. Combing the records, in which chivalry is, indeed, little mentioned, they have succeeded in presenting a picture of the Middle Ages in which economic and social points of view are so dominant that one tends at times to forget that, next to religion, chivalry was the strongest of the ideas that filled the minds and hearts of those men of another age."

9. A. Touraine, *Sociologie de l'action* (Paris: Editions du Seuil, 1965), chap. VI, "The Political System," p. 298. This two-fold fragility consists partly in the danger that the study of political relationships may be absorbed by structural analysis on the one hand and history on the other; partly in the fact that political theory may be subject either to politics or to political philosophy, itself only a part of the philosophy of history.

10. E. Morin, *Introduction à une politique de l'homme* (Paris: Editions du Seuil, 1965), new ed., 1969, pp. 9-10: *La politique en miettes.*

11. In English the emergence of two terms, "policy" and "polity" (in the fourteenth century the French had tried out *policie,* also copied from the Greek, but it did not take), complicated the field of political science and incidentally that of political history. While the French philosophes of the eighteenth century sought, or accepted, a compromise between philosophical and political history, it may be that in England an even more radical dilemma caused an oscillation between historical and political, at once linked and opposed to each other. This possibility seems suggested by such titles as that published anonymously in London in 1706: *An Historical and Political Essay, Discussing the Affinity or Resemblance of the Ancient and Modern Government.* See J. A. W. Gunn, "The 'Civil Polity' of Peter Paxton," *Past and Present,* 40 (July 1968), 56.

12. The best example is the preface to the *Etudes historiques* (1831).

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13. This approach was set out in the *Cours d'histoire moderne: histoire de la civilisation en Europe depuis la chute de l'Empire Romain jusqu'à la Révolution Française* (1828), lecture I. For long passages from Chateaubriand and Guizot, see J. Ehrard and G. Palmade, *L'histoire* (Paris: Colin, 1969), pp. 189-193, 203-207.

14. R. Aron, "Thucydide et le récit historique," *Theory and History,* 1, no. 2 (1960), reprinted in *Dimensions de la conscience historique* (Paris: Plon, 1961), pp. 147-197.

15. As throughout this article, the works quoted on medieval history are meant only as references and examples, not as a bibliography or selection in terms of merit.

16. Aron, "Thucydide et le récit historique," in *Dimensions,* p. 189.

17. An example of a traditional but all the same very pertinent study is F. M. Powicke, "Reflections on the Medieval State," *Transactions of the Royal Historical Society,* ser. 4, XIX (1936). Among new approaches are: B. Guenée, "L'histoire de l'état en France à la fin du Moyen Age vue par les historiens français depuis cent ans," *Revue historique,* 232 (1964), 331-360; "Etat et nation en France au Moyen Age," *ibid.,* 237 (1967), 17-30; "Espace et état dans la France du bas Moyen Age," *Annales: économies, sociétés, civilisations* (1968), pp. 744-758. It will be noted that the word "power" (accompanied, it is true, by an adjective) occurs in the title of the pioneer work by E. Lavisse, "Etude sur le pouvoir royal au temps de Charles V," *Revue historique* (1884), pp. 233-280, which attempts to go beyond the description of institutions to mental realities. Marc Bloch noted the connection between the history of the state and the history of a nation or nations. "It seems to be difficult to separate the history of the idea of the state from the history of the idea of nation, or patriotism." *Revue historique,* 128 (1918), 347.

18. The way Marxists tended to concentrate their interest on the state is apparent from the titles of their works: for example, F. Engels, *Private Property and the State*; V. I. Lenin, *State and Revolution.* On the two senses of "nation" in Marx and Engels (the modern designating "a kind of rising capitalism," and the other the more general Latin sense of ethnic group) see A. Pelletier and J. J. Goblot, *Materialisme historique et histoire des civilisations* (Paris: Editions sociales, 1969), pp. 94ff.

19. Marc Bloch, *Mélanges d'histoire sociale* (1944), p. 120, cited by Guenée, "L'histoire de l'etat en France," p. 345.

20. *Schriften der Monumenta Germaniae Historica,* XIII, 3 vols. (Stuttgart: Hiersemann, 1954-1956).

21. P. E. Schramm summed up the position himself in the résumé of his contribution to the Rome Conference of 1955: "Die Staatsymbolik des Mittelalters," in *X Congresso Internazionale di Scienze storiche* (Rome, 1955), vol. VII, *Riassunti delle communicazioni,* pp. 200-201.

22. M. Hellmann, ed. (Weimar: Böhlau, 1961). Among many studies on the

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symbolism of the crown in the Middle Ages, see pp. 336-383, "The Crown as Fiction," in E. H. Kantorowicz, *The King's Two Bodies: A Study in Medieval Political Theology* (Princeton: Princeton University Press, 1957).

23. "It is not by chance that the relic St. Louis brought to Paris and installed in the chapel of his palace is a crown of thorns, doubly symbolic of kingship and of sacrifice." *Le Monde,* April 29, 1970, p. 13.

24. Schramm, "Die Staatsymbolik des Mittelalters," pp. 200-201.

25. Robert Folz, *L'idée d'empire en occident du Ve au XIVe siècles* (Paris: Aubier, 1953), p. 6.

26. On kingship in the early Middle Ages see especially J. M. Wallace-Hadrill, *The Long-Haired Kings* (London: Methuen, 1962), and F. Graus, *Volk, Herrscher und Heiliger in Reich der Merowinger* (Prague, 1965). For the Carolingian period, see the recent study by W. Ullmann, *The Carolingian Renaissance and the Idea of Kingship* (London: Methuen, 1969), which brings out especially well (p. 17) how then, "in conformity to and in accordance with the basic premisses of the ecclesiological theme and the wholeness of view, there was no conceptual distinction between a Carolingian State and a Carolingian Church." Georges Duby stressed the importance of the royal model within the feudal system at the international symposium, *Problèmes de stratification sociale,* 1966, published by R. Mousnier, Publications of the Faculté des Lettres et Sciences Humaines de Paris, Sorbonne, 'Recherches," XLIII (Paris, 1968). See K. Gorski, "Le roi-saint: problème d'idéologie féodale," *Annales: économies, sociétés, civilisations* (1969), pp. 370-376.

27. *Atti dell' VIII Congresso Internazionale di Storia delle religioni* (Florence, 1956).

28. *Studies in the History of Religions,* supplements to NVMEN IV, *The Sacral Kingship: La Regalità Sacra* (Leyden, 1959). Out of fifty-six contributions, only four are devoted to the Middle Ages in the West: M. Maccarrone, "Il sovrano 'Vicarius Dei' nell'alto medio evo," pp. 581-594; M. Murray, "The Divine King," pp. 595-608; L. Rougier, "Le caractère sacré de la royauté en France," pp. 609-619; and J. A. Bizet, "La notion du royaume intérieur chez les mystiques germaniques du XIVe siècle," pp. 620-626.

29. E. H. Kantorowicz, *Kaiser Friedrich der Zweite* (Berlin: Bondi, 1927), and *Ergänzungsband* (Berlin: Bondi, 1931).

30. E. H. Kantorowicz, *Laudes Regiae: A Study in Liturgical Acclamations and Medieval Ruler Worship* (Berkeley and Los Angeles: University of California Press, 1946).

31. E. H. Kantorowicz, *The King's Two Bodies: A Study in Medieval Political Theology* (Princeton: Princeton University Press, 1957). See also the reviews by R. W. Southern in the *Journal of Ecclesiastical History,* 10 (1957), and B. Smalley in *Past and Present,* no. 20 (November 1961).

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32. Sir James George Frazer, *The Golden Bough* (London: Macmillan, 1890), part I: "The Magic Art and the Evolution of Kings." Frazer, *Lectures on the Early History of Kingship* (London: Macmillan, 1905).

33. A medievalist, Joseph R. Strayer, wrote an essay on "The Historian's Concept of Public Opinion" in the collection edited by M. Komarovsky, *Common Frontiers of the Social Sciences* (Glencoe, Ill.: Free Press, 1957). Marvin B. Becker, "Dante and His Literary Contemporaries as Political Men," *Speculum* (1966), p. 674, n. 28, calls attention to "the neglected theme of the language and imagery of medieval politics," and quotes the article by E. H. Kantorowicz, "Christus-Fiscus," in *Synopsis: Festgabe für Alfred Weber* (Heidelberg: Schneider, 1948), pp. 225-235.

34. "Anthropologie politique" is the title of an informative essay by Georges Balandier, 1967. He sets out systematically what E. R. Leach has observed to be "contradictory, conflictual, approximate, and externally relative" in societies, developing the theme of E. E. Evans-Pritchard in "Anthropology and History," 1961.

35. Here again there is an incompatibility between the point of view of Freund, *L'essence du politique*, p. 538, according to which "the class struggle is only an aspect of the political struggle," and the Marxist point of view, according to which all forms of political struggle derive from the class struggle. As long as it is not applied too dogmatically and inflexibly, I think the Marxist view is the truer and more fruitful. G. Cracco's stimulating book, *Società e Stato nel medievo veneziano (secoli XII-XIV)* (Florence: Olschki, 1967), shows the class struggle functioning normally in the political history of Venice, usually thought to be a world apart. It may be thought, however, that the author is limited by an approach based too much on the idea of the state. F. C. Lane makes reservations of this kind in a generally appreciative review in *Speculum* (1968), pp. 497-501.

36. See especially K. Bosl, "Potens und Pauper: Begriffgeschichtliche Studien zur gesellschaftlichen Differenzierung im frühen Mittelalter und zum 'Pauperismus' des Hochmittelalter," in *Alteuropa und die moderne Gesellschaft: Festschrift für Otto Brunner* (Göttingen: Vandenhoeck and Ruprecht, 1963), pp. 60-87, reprinted in *Frühformen der Gesellschaft im mittelalterlichen Europa* (Munich-Vienna: Oldenbourg, 1964), pp. 106-134. Also J. Le Goff, "Le vocabulaire des catégories sociales chez Saint François d'Assise et ses premiers biographes," in the international symposium organized by the Ecole Normale Supérieure of Saint-Cloud, 1967, on the vocabulary of social classes (in press).

37. G. Duby's conception of *seigneurie banale* is set out in his thesis, *La société aux XIe et XIIe siècles dans la région maconnaise* (Paris: Colin, 1953), and in *L'économie rurale et la vie des compagnes dans l'Occident medieval* (Paris: Aubier, 1962), vol. II, bk. III, "XI-XIIIe siècles: la seigneurie et l'économie rurale." In the legally oriented series, the *Recueils de la Société Jean Bodin*, the volume *Gouvernants et Gouvernés*, XXV (1965), shows a preoccupation with the themes of power which may

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derive from Marc Bloch, *La société féodale* (Paris: Michel, 1939), vol. II, bk. 2, "Le gouvernement des hommes." The theme also occurs in J. Dhondt, "'Ordres' ou 'puissances': l'exemple des états de Flandre," *Annales: économies, sociétés, civilisations* (1950), pp. 289-305.

38. See H. Grundmann, "Litteratus-Illitteratus: Der Wandlung einer Bildungsnorm vom Altertum zum Mittelalter," *Archiv für Kulturgeschichte,* 40 (1958), and his "Sacerdotium-Regnum-Studium: zur Wertung der Wissenschaft im 13. Jahrhundert," *Archiv für Kulturgeschichte,* 34 (1951).

39. See my *Les intellectuels au Moyen Age* (Paris: Editions du Seuil, 1957), for an attempt to show how, between the end of the twelfth and the fourteenth century, members of the universities moved from a socioprofessional position which was corporative to one which placed them among the possessors of power.

40. The subject proposed by the French delegation to the International University Committee on History at the Thirteenth International Conference on Historical Sciences in Moscow, August 1970. I believe Professor Lawrence Stone has a similar project in mind for English universities in the modern era. This revival of interest in the prosopographical method, a method of social history likely to favor the renewal of political history, is evident in various sectors (see the late 1970 number of *Annales: économies, sociétés, civilisations*).

41. Among G. Dumézil's many fascinating studies on the tri-functional ideology of the Indo-Europeans, one of the most recent is *Idées romaines* (Paris: Gallimard, 1969), in which he poses various questions about western Europe in the Middle Ages. Two examples of initial research in this field are J. Batany, "Des 'Trois Fonctions' aux 'Trois Etats'?" *Annales: économies, sociétés, civilisations* (1936), pp. 933-938, and J. Le Goff, "Note sur société tripartie, idéologie monarchique et renouveau économique dans la chrétienté du IXe au XIIe siècle," in T. Manteuffel and A. Gieysztor, eds., *L'Europe aux IX-XIe siècles* (Warsaw, 1968), pp. 63-71.

42. One of the works inspired by this particular question is Joan Evans' interesting *Art in Medieval France, 987-1498: A Study in Patronage* (London: Oxford University Press, 1948).

43. E. Panofsky, *Gothic Architecture and Scholasticism* (New York: Meridian, 1957). A more traditional view is given in R. Branner, *St. Louis and the Court Style in Gothic Architecture* (London: Zwemmer, 1965).

44. On the significance of Botticelli's *Primavera* see P. Francastel, *La realité figurative* (Paris: Gonthier, 1965), p. 241, "La fête mythologique au Quattrocentro," and p. 272, "Un mythe politique et social du Quattrocento." See Ernst Gombrich, *Botticelli's Mythologies: A Study of the Neoplatonic Symbolism of Its Circle,* in *Journal of the Warburg and Courtauld Institutes* (1945). P. Francastel has developed these ideas in *La figure et le lieu: l'ordre visuel du Quattrocento* (Paris: Gallimard, 1967).

45. See R. Manselli, *L'eresia del male* (Naples: Morano, 1963), and "Les

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hérétiques dans la société italienne du XIIIe siècle," in *Hérésies et sociétés dans l'Europe pré-industrielle, XIe-XVIIIe siècles,* a Royaumont symposium, presented by J. Le Goff (Paris and The Hague, 1968), pp. 199-202. This points out the "very close link between the Catharist heresy and the great political party of the Ghibellines." This study needs to be developed in the direction of a sociological comparison between religious sect and political party.

46. I will limit myself to references to the international symposium in Amsterdam in 1967, "Urban Core and Inner City"; Nelson W. Polsby, *Community Power and Political Theory* (New Haven: Yale University Press, 1963); and the "anti-historicist" works of Manuel Castells, which include "Le centre urbain: projet de recherche sociologique," in *Cahiers internationaux de sociologie* (1969), pp. 83-106, and "Vers une theorie sociologique de la planification urbaine," in *Sociologie du travail* (1969), pp. 414-443. All these deal with the modern period.

47. W. Braunfels, *Mittelalterliche Stadtbaukunst der Toskana* (Berlin, 1953).

48. See especially the preface to F. Braudel, *La Méditerranée et le monde méditerranéen à l'époque de Philippe II* (Paris: Colin, 1949), revised and augmented 2d ed., 1966; the idea is repeated in *Ecrits sur l'histoire* (Paris: Flammarion, 1969), pp. 11-13.

49. L. Febvre, *Combats pour l'histoire* (Paris: Colin, 1953), p. 118 (written in 1947).

50. *Ibid.,* p. 20 (written in 1941).

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E. J. HOBSBAWM

From Social History to the History of Society

THIS ESSAY is an attempt to observe and analyze, not to state a personal credo or to express (except where this is clearly stated) the author's preferences and value judgments. I say this at the outset in order to distinguish this essay from others which are defenses of or pleas for the kind of history practiced by their authors—as it happens social history does not need either at the moment—but also to avoid two misunderstandings especially common in discussions heavily charged with ideology. All discussions about social history are.

The first is the tendency for readers to identify authors with the views they write about, unless they disclaim this identification in the clearest terms and sometimes even when they do so. The second is the tendency to confuse the ideological or political motivations of research, or its utilization, with its scientific value. Where ideological intention or bias produces triviality or error, as is often the case in the human sciences, we may happily condemn motivation, method, and result. However, life would be a great deal simpler if our understanding of history were advanced exclusively by those with whom we are in agreement or in sympathy on all public and even private matters. Social history is at present in fashion. None of those who practice it would care to be seen keeping ideological company with all those who come under the same historical heading. Nevertheless, what is more important than to define one's attitude is to discover where social history stands today after two decades of unsystematic if copious development, and whither it might go.

I

The term social history has always been difficult to define, and until recently there has been no great pressure to define it, for it has lacked the institutional and professional vested interests which

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normally insist on precise demarcations. Broadly speaking, until the present vogue of the subject—or at least of the name—it was in the past used in three sometimes overlapping senses. First, it referred to the history of the poor or lower classes, and more specifically to the history of the movements of the poor ("social movements"). The term could be even more specialized, referring essentially to the history of labor and socialist ideas and organizations. For obvious reasons this link between social history and the history of social protest or socialist movements has remained strong. A number of social historians have been attracted to the subject because they were radicals or socialists and as such interested in subjects of great sentimental relevance to them.[1]

Second, the term was used to refer to works on a variety of human activities difficult to classify except in such terms as "manners, customs, everyday life." This was, perhaps for linguistic reasons, a largely Anglo-Saxon usage, since the English language lacks suitable terms for what the Germans who wrote about similar subjects—often also in a rather superficial and journalistic manner—called *Kultur-* or *Sittengeschichte*. This kind of social history was not particularly oriented toward the lower classes—indeed rather the opposite—though the more politically radical practitioners tended to pay attention to them. It formed the unspoken basis of what may be called the residual view of social history, which was put forward by the late G. M. Trevelyan in his *English Social History* (London, 1944) as "history with the politics left out." It requires no comment.

The third meaning of the term was certainly the most common and for our purposes the most relevant: "social" was used in combination with "economic history." Indeed, outside the Anglo-Saxon world, the title of the typical specialist journal in this field before the Second World War always (I think) bracketed the two words, as in the *Vierteljahrschrift fuer Sozial u. Wirtschaftsgeschichte,* the *Revue d'Histoire E. & S.*, or the *Annales d'Histoire E. & S.* It must be admitted that the economic half of this combination was overwhelmingly preponderant. There were hardly any social histories of equivalent caliber to set beside the numerous volumes devoted to the economic history of various countries, periods, and subjects. There were in fact not very many economic and social histories. Before 1939 one can think of only a few such works, admittedly sometimes by impressive authors (Pirenne, Mikhail Rostovtzeff, J. W. Thompson, perhaps Dopsch), and the mono-

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graphic or periodical literature was even sparser. Nevertheless, the habitual bracketing of economic and social, whether in the definitions of the general field of historical specialization or under the more specialized banner of economic history, is significant.

It revealed the desire for an approach to history systematically different from the classical Rankean one. What interested historians of this kind was the evolution of the economy, and this in turn interested them because of the light it threw on the structure and changes in society, and more especially on the relationship between classes and social groups, as George Unwin admitted.[2] This social dimension is evident even in the work of the most narrowly or cautiously economic historians so long as they claimed to be historians. Even J. H. Clapham argued that economic history was of all varieties of history the most fundamental because it was the foundation of society. The predominance of the economic over the social in this combination had, we may suggest, two reasons. It was partly owing to a view of economic theory which refused to isolate the economic from social, institutional, and other elements, as with the Marxists and the German historical school, and partly to the sheer headstart of economics over the other social sciences. If history had to be integrated into the social sciences, economics was the one it had primarily to come to terms with. One might go further and argue (with Marx) that, whatever the essential inseparability of the economic and the social in human society, the analytical base of any historical inquiry into the evolution of human societies must be the process of social production.

None of the three versions of social history produced a specialized academic field of social history until the 1950's, though at one time the famous *Annales* of Lucien Febvre and Marc Bloch dropped the economic half of its subtitle and proclaimed itself purely social. However, this was a temporary diversion of the war years, and the title by which this great journal has now been known for a quarter of a century—*Annales: économies, sociétés, civilisations*—as well as the nature of its contents, reflect the original and essentially global and comprehensive aims of its founders. Neither the subject itself, nor the discussion of its problems, developed seriously before 1950. The journals specializing in it, still few in number, were not founded until the end of the 1950's: we may perhaps regard the *Comparative Studies in Society and History* (1958) as the first. As an academic specialization, social history is therefore quite new.

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What explains the rapid development and growing emancipation of social history in the past twenty years? The question could be answered in terms of technical and institutional changes within the academic disciplines of social science: the deliberate specialization of economic history to fit in with the requirements of the rapidly developing economic theory and analysis, of which the "new economic history" is an example; the remarkable and worldwide growth of sociology as an academic subject and fashion, which in turn called for subsidiary historical service-branches analogous to those required by economics departments. We cannot neglect such factors. Many historians (such as the Marxists) who had previously labeled themselves economic because the problems they were interested in were plainly not encouraged or even considered by orthodox general history, found themselves extruded from a rapidly narrowing economic history and accepted or welcomed the title of "social historians," especially if their mathematics were poor. It is improbable whether in the atmosphere of the 1950's and early 1960's someone like R. H. Tawney would have been welcomed among the economic historians had he been a young researcher and not president of the Economic History Society. However, such academic redefinitions and professional shifts hardly explain much, though they cannot be overlooked.

Far more significant was the general historization of the social sciences which took place during this period, and may retrospectively appear to have been the most important development within them at this time. For my present purpose it is not necessary to explain this change, but it is impossible to avoid drawing attention to the immense significance of the revolutions and struggles for political and economic emancipation of colonial and semicolonial countries, which drew the attention of governments, international and research organizations, and consequently also of social scientists, to what are essentially problems of historic transformations. These were subjects which had hitherto been outside, or at best on the margins of, academic orthodoxy in the social sciences, and had increasingly been neglected by historians.

At all events essentially historical questions and concepts (sometimes, as in the case of "modernization" or "economic growth," excessively crude concepts) have captured even the discipline hitherto most immune to history, when not actually, like Radcliffe-Brown's social anthropology, actively hostile to it. This progressive infiltration of history is perhaps most evident in economics,

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where an initial field of growth economics, whose assumptions, though much more sophisticated, were those of the cookery book ("Take the following quantities of ingredients *a* through *n*, mix and cook, and the result will be the take-off into self-sustained growth"), has been succeeded by the growing realization that factors outside economics also determine economic development. In brief, it is now impossible to pursue many activities of the social scientist in any but a trivial manner without coming to terms with social structure and its transformations: without the history of societies. It is a curious paradox that the economists were beginning to grope for some understanding of social (or at any rate not strictly economic) factors at the very moment when the economic historians, absorbing the economists' models of fifteen years earlier, were trying to make themselves look hard rather than soft by forgetting about everything except equations and statistics.

What can we conclude from this brief glance at the historical development of social history? It can hardly be an adequate guide to the nature and tasks of the subject under consideration, though it can explain why certain more or less heterogeneous subjects of research came to be loosely grouped under this general title, and how developments in other social sciences prepared the ground for the establishment of an academic theory specially demarcated as such. At most it can provide us with some hints, at least one of which is worth mentioning immediately.

A survey of social history in the past seems to show that its best practitioners have always felt uncomfortable with the term itself. They have either, like the great Frenchmen to whom we owe so much, preferred to describe themselves simply as historians and their aim as "total" or "global" history, or as men who sought to integrate the contributions of all relevant social sciences in history, rather than to exemplify any one of them. Marc Bloch, Fernand Braudel, Georges Lefebvre are not names which can be pigeonholed as social historians except insofar as they accepted Fustel de Coulanges' statement that "History is not the accumulation of events of all kinds which occurred in the past. It is the science of human societies."

Social history can never be another specialization like economic or other hyphenated histories because its subject matter cannot be isolated. We can define certain human activities as economic, at least for analytical purposes, and then study them historically. Though this may be (except for certain definable purposes) artifi-

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cial or unrealistic, it is not impracticable. In much the same way, though at a lower level of theory, the old kind of intellectual history which isolated written ideas from their human context and traced their filiation from one writer to another is possible, if one wants to do that sort of thing. But the social or societal aspects of man's being cannot be separated from the other aspects of his being, except at the cost of tautology or extreme trivialization. They cannot, for more than a moment, be separated from the ways in which men get their living and their material environment. They cannot, even for a moment, be separated from their ideas, since their relations with one another are expressed and formulated in language which implies concepts as soon as they open their mouths. And so on. The intellectual historian may (at his risk) pay no attention to economics, the economic historian to Shakespeare, but the social historian who neglects either will not get far. Conversely, while it is extremely improbable that a monograph on provençal poetry will be economic history or one on inflation in the sixteenth century intellectual history, both could be treated in a way to make them social history.

II

Let us turn from the past to the present and consider the problems of writing the history of society. The first question concerns how much societal historians can get from other social sciences, or indeed how far their subject is or ought to be merely the science of society insofar as it deals with the past. This question is natural, though the experience of the past two decades suggests two different answers to it. It is clear that social history has since 1950 been powerfully shaped and stimulated, not only by the professional structure of other social sciences (for example, their specific course requirements for university students), and by their methods and techniques, but also by their questions. It is hardly too much to say that the recent efflorescence of studies in the British industrial revolution, a subject once grossly neglected by its own experts because they doubted the validity of the concept of industrial revolution, is due primarily to the urge of economists (doubtless in turn echoing that of governments and planners) to discover how industrial revolutions happen, what makes them happen, and what sociopolitical consequences they have. With certain notable exceptions, the flow of stimulation in the past twenty years has been one

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way. On the other hand, if we look at recent developments in another way, we shall be struck by the obvious convergence of workers from different disciplines toward sociohistorical problems. The study of millennial phenomena is a case in point, since among writers on these subjects we find men coming from anthropology, sociology, political science, history, not to mention students of literature and religions—though not, so far as I am aware, economists. We also note the transfer of men with other professional formations, at least temporarily, to work which historians would consider historical—as with Charles Tilly and Neil Smelser from sociology, Eric Wolf from anthropology, Everett Hagen and Sir John Hicks from economics.

Yet the second tendency is perhaps best regarded not as convergence but as conversion. For it must never be forgotten that if nonhistorical social scientists have begun to ask properly historical questions and to ask historians for answers, it is because they themselves have none. And if they have sometimes turned themselves into historians, it is because the practicing members of our discipline, with the notable exception of the Marxists and others—not necessarily *Marxisants*—who accept a similar problematic, have not provided the answers.[5] Moreover, though there are now a few social scientists from other disciplines who have made themselves sufficiently expert in our field to command respect, there are more who have merely applied a few crude mechanical concepts and models. For every *Vendée* by a Tilly, there are, alas, several dozen equivalents of Rostow's *Stages*. I leave aside the numerous others who have ventured into the difficult territory of historical source material without an adequate knowledge of the hazards they are likely to encounter there, or of the means of avoiding and overcoming them. In brief, the situation at present is one in which historians, with all their willingness to learn from other disciplines, are required to teach rather than to learn. The history of society cannot be written by applying the meager available models from other sciences; it requires the construction of adequate new ones—or, at least (Marxists would argue), the development of existing sketches into models.

This is not, of course, true of techniques and methods, where the historians are already net debtors to a substantial extent, and will, or at least ought, to go even more heavily and systematically into debt. I do not wish to discuss this aspect of the problem of the history of society, but a point or two can be made in passing. Given

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the nature of our sources, we can hardly advance much beyond a combination of the suggestive hypothesis and the apt anecdotal illustration without the techniques for the discovery, the statistical grouping, and handling of large quantities of data, where necessary with the aid of division of research labor and technological devices, which other social sciences have long developed. At the opposite extreme, we stand in equal need of the techniques for the observation and analysis in depth of specific individuals, small groups, and situations, which have also been pioneered outside history, and which may be adaptable to our purposes—for example, the participant observation of the social anthropologists, the interview-in-depth, perhaps even psychoanalytical methods. At the very least these various techniques can stimulate the search for adaptations and equivalents in our field, which may help to answer otherwise impenetrable questions.[6]

I am much more doubtful about the prospect of turning social history into a backward projection of sociology, as of turning economic history into retrospective economic theory, because these disciplines do not at present provide us with useful models or analytical frameworks for the study of long-run *historical* socio-economic transformations. Indeed the bulk of their thinking has not been concerned with, or even interested in, such changes, if we except such trends as Marxism. Moreover, it may be argued that in important respects their analytical models have been developed systematically, and most profitably, by abstracting from historical change. This is notably true, I would suggest, of sociology and social anthropology.

The founding fathers of sociology have indeed been more historically minded than the main school of neoclassic economics (though not necessarily than the original school of classical political economists), but theirs is an altogether less developed science. Stanley Hoffmann has rightly pointed to the difference between the "models" of the economists and the "checklists" of the sociologists and anthropologists.[7] Perhaps they are more than mere checklists. These sciences have also provided us with certain visions, patterns of possible structures composed of elements which can be permuted and combined in various ways, vague analogues to Kekulé's ring glimpsed at the top of the bus, but with the drawback of unverifiability. At their best such structural-functional patterns may be both elegant and heuristically useful, at least for some. At a more modest

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level, they may provide us with useful metaphors, concepts, or terms (such as "role"), or convenient aids in ordering our material.

Moreover, quite apart from their deficiency as models, it may be argued that the theoretical constructions of sociology (or social anthropology) have been most successful by excluding history, that is, directional or oriented change.[8] Broadly speaking, the structural-functional patterns illuminate what societies have in common in spite of their differences, whereas our problem is with what they have not. It is not what light Lévi-Strauss's Amazonian tribes can throw on modern (indeed on any) society, but on how humanity got from the cavemen to modern industrialism or postindustrialism, and what changes in society were associated with this progress, or necessary for it to take place, or consequential upon it. Or, to use another illustration, it is not to observe the permanent necessity of all human societies to supply themselves with food by growing or otherwise acquiring it, but what happens when this function, having been overwhelmingly fulfilled (since the neolithic revolution) by classes of peasants forming the majority of their societies, comes to be fulfilled by small groups of other kinds of agricultural producers and may come to be fulfilled in nonagricultural ways. How does this happen and why? I do not believe that sociology and social anthropology, however helpful they are incidentally, at present provide us with much guidance.

On the other hand, while I remain skeptical of most current economic theory as a framework of the historical analysis of societies (and therefore of the claims of the new economic history), I am inclined to think that the possible value of economics for the historian of society is great. It cannot but deal with what is an essentially dynamic element in history, namely the process—and, speaking globally and on a long time-scale, progress—of social production. Insofar as it does this it has, as Marx saw, historical development built into it. To take a simple illustration: the concept of the "economic surplus," which the late Paul Baran revived and utilized to such good effect,[9] is patently fundamental to any historian of the development of societies, and strikes me as not only more objective and quantifiable, but also more primary, speaking in terms of analysis, than, say, the dichotomy *Gemeinschaft-Gesellschaft.* Of course Marx knew that economic models, if they are to be valuable for historical analysis, cannot be divorced from social and institutional realities, which include certain basic types of human communal or kinship organization, not to mention the structures and

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assumptions specific to particular socioeconomic formations or cultures. And yet, though Marx is not for nothing regarded as one of the major founding fathers of modern sociological thought (directly and through his followers and critics), the fact remains that his major intellectual project *Das Kapital* took the form of a work of economic analysis. We are required neither to agree with his conclusions nor his methodology. But we would be unwise to neglect the practice of the thinker who, more than any other, has defined or suggested the set of historical questions to which social scientists find themselves drawn today.

III

How are we to write the history of society? It is not possible for me to produce a definition or model of what we mean by society here, or even a checklist of what we want to know about its history. Even if I could, I do not know how profitable this would be. However, it may be useful to put up a small and miscellaneous assortment of signposts to direct or warn off future traffic.

(1) The history of society is *history;* that is to say it has real chronological time as one of its dimensions. We are concerned not only with structures and their mechanisms of persistence and change, and with the general possibilities and patterns of their transformations, but also with what actually happened. If we are not, then (as Fernand Braudel has reminded us in his article on "Histoire et Longue Durée"[10]) we are not historians. *Conjectural* history has a place in our discipline, even though its chief value is to help us assess the possibilities of present and future, rather than past, where its place is taken by *comparative* history; but actual history is what we must explain. The possible development or nondevelopment of capitalism in imperial China is relevant to us only insofar as it helps to explain the actual fact that this type of economy developed fully, at least to begin with, in one and only one region of the world. This in turn may be usefully contrasted (again in the light of general models) with the tendency for other systems of social relations—for example, the broadly feudal—to develop much more frequently and in a greater number of areas. The history of society is thus a collaboration between general models of social structure and change and the specific set of phenomena which actually occurred. This is true whatever the geographical or chronological scale of our inquiries.

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(2) The history of society is, among other things, that of specific units of people living together and definable in sociological terms. It is the history of societies as well as of human society (as distinct from, say, that of apes and ants), or of certain types of society and their possible relationships (as in such terms as "bourgeois" or "pastoral" society), or of the general development of humanity considered as a whole. The definition of a society in this sense raises difficult questions, even if we assume that we are defining an objective reality, as seems likely, unless we reject as illegitimate such statements as "Japanese society in 1930 differed from English society." For even if we eliminate the confusions between different uses of the word "society," we face problems (a) because the size, complexity, and scope of these units varies, for example, at different historical periods or stages of development; and (b) because what we call society is merely one set of human interrelations among several of varying scale and comprehensiveness into which people are classifiable or classify themselves, often simultaneously and with overlaps. In extreme cases such as New Guinea or Amazon tribes, these various sets may define the same group of people, though this is in fact rather improbable. But normally this group is congruent neither with such relevant sociological units as the community, nor with certain wider systems of relationship of which the society forms a part, and which may be functionally essential to it (like the set of economic relations) or nonessential (like those of culture).

Christendom or Islam exist and are recognized as self-classifications, but though they may define a *class* of societies sharing certain common characteristics, they are not societies in the sense in which we use the word when talking about the Greeks or modern Sweden. On the other hand, while in many ways Detroit and Cuzco are today part of a single system of functional interrelationships (for example, part of one economic system), few would regard them as part of the same society, sociologically speaking. Neither would we regard as one the societies of the Romans or the Han and those of the barbarians who formed, quite evidently, part of a wider system of interrelationships with them. How do we define these units? It is far from easy to say, though most of us solve—or evade—the problem by choosing some outside criterion: territorial, ethnic, political, or the like. But this is not always satisfactory. The problem is more than methodological. One of the major themes of the history of modern societies is the increase in their scale, internal homo-

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geneity, or at least in the centralization and directness of social relationships, the change from an essentially pluralist to an essentially unitary structure. In tracing this, problems of definition become very troublesome, as every student of the development of national societies or at least of nationalisms knows.

(3) The history of societies requires us to apply, if not a formalized and elaborate model of such structures, then at least an approximate order of research priorities and a working assumption about what constitutes the central nexus or complex of connections of our subject, though of course these things imply a model. Every social historian does in fact make such assumptions and hold such priorities. Thus I doubt whether any historian of eighteenth-century Brazil would give the Catholicism of that society analytical priority over its slavery, or any historian of nineteenth-century Britain would regard kinship as central a social nexus as he would in Anglo-Saxon England.

A tacit consensus among historians seems to have established a fairly common working model of this kind, with variants. One starts with the material and historical environment, goes on to the forces and techniques of production (demography coming somewhere in between), the structure of the consequent economy—divisions of labor, exchange, accumulation, distribution of the surplus, and so forth—and the social relations arising from these. These might be followed by the institutions and the image of society and its functioning which underlie them. The shape of the social structure is thus established, the specific characteristics and details of which, insofar as they derive from other sources, can then be determined, most likely by comparative study. The practice is thus to work outwards and upwards from the process of social production in its specific setting. Historians will be tempted—in my view rightly—to pick on one particular relation or relational complex as central and specific to the society (or type of society) in question, and to group the rest of the treatment around it—for example, Bloch's "relations of interdependence" in his *Feudal Society,* or those arising out of industrial production, possibly in industrial society, certainly in its capitalist form. Once the structure has been established, it must be seen in its historical movement. In the French phrase "structure" must be seen in "conjuncture," though this term must not be taken to exclude other, and possibly more relevant, forms and patterns of historical change. Once again the tendency is to treat economic movements (in the broadest

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sense) as the backbone of such an analysis. The tensions to which the society is exposed in the process of historic change and transformation then allow the historian to expose (1) the general mechanism by which the structures of society simultaneously tend to lose and reestablish their equilibria, and (2) the phenomena which are traditionally the subject of interest to the social historians, for example, collective consciousness, social movements, the social dimension of intellectual and cultural changes, and so on.

My object in summarizing what I believe—perhaps wrongly—to be a widely accepted working plan of social historians is not to recommend it, even though I am personally in its favor. It is rather the opposite: to suggest that we try and make the implicit assumptions on which we work explicit and to ask ourselves whether this plan is in fact the best for the formulation of the nature and structure of societies and the mechanisms of their historic transformations (or stabilizations), whether other plans of work based on other questions can be made compatible with it, or are to be preferred to it, or can simply be superimposed to produce the historical equivalent of those Picasso portraits which are simultaneously displayed full-face and in profile.

In brief, if as historians of society we are to help in producing—for the benefit of all the social sciences—valid models of sociohistoric dynamics, we shall have to establish a greater unity of our practice and our theory, which at the present stage of the game probably means in the first instance to watch what we are doing, to generalize it, and to correct it in the light of the problems arising out of further practice.

IV

Consequently, I should like to conclude by surveying the actual practice of social history in the past decade or two, in order to see what future approaches and problems it suggests. This procedure has the advantage that it fits in both with the professional inclinations of a historian and with what little we know about the actual progress of sciences. What topics and problems have attracted most attention in recent years? What are the growing-points? What are the interesting people doing? The answers to such questions do not exhaust analysis, but without them we cannot get very far. The consensus of workers may be mistaken, or distorted by fashion or—as is obviously the case in such a field as the study of public disorder

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—by the impact of politics and administrative requirements, but we neglect it at our peril. The progress of science has derived less from the attempt to define perspectives and programs a priori—if it did we should now be curing cancer—than from an obscure and often simultaneous convergence upon the questions worth asking and, above all, those ripe for an answer. Let us see what has been happening, at least insofar as it is reflected in the impressionistic view of one observer.

Let me suggest that the bulk of interesting work in social history in the past ten or fifteen years has clustered around the following topics or complexes of questions:

(1) Demography and kinship
(2) Urban studies insofar as these fall within our field
(3) Classes and social groups
(4) The history of "mentalities" or collective consciousness or of "culture" in the anthropologists' sense
(5) The transformation of societies (for example, modernization or industrialization)
(6) Social movements and phenomena of social protest.

The first two groups can be singled out because they have already institutionalized themselves as fields, regardless of the importance of their subject matter, and now possess their own organization, methodology, and system of publications. Historical demography is a rapidly growing and fruitful field, which rests not so much on a set of problems as on a technical innovation in research (family reconstitution) that makes it possible to derive interesting results from material hitherto regarded as recalcitrant or exhausted (parish registers). It has thus opened a new range of sources, whose characteristics in turn have led to the formulation of questions. The major interest for social historians of historical demography lies in the light it sheds on certain aspects of family structure and behavior, on the life-curves of people at different periods, and on intergenerational changes. These are important though limited by the nature of the sources—more limited than the most enthusiastic champions of the subject allow, and certainly by themselves insufficient to provide the framework of analysis of "The World We Have Lost." Nevertheless, the fundamental importance of this field is not in question, and it has served to encourage the use of strict quantitative techniques. One welcome effect—or side effect—has been to arouse a greater interest in historical problems of

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kinship structure than social historians might have shown without this stimulus, though a modest demonstration effect from social anthropology ought not to be neglected. The nature and prospects of this field have been sufficiently debated to make further discussion unnecessary here.

Urban history also possesses a certain technologically determined unity. The individual city is normally a geographically limited and coherent unit, often with its specific documentation and even more often of a size which lends itself to research on the Ph.D. scale. It also reflects the urgency of urban problems which have increasingly become the major, or at least the most dramatic, problems of social planning and management in modern industrial societies. Both these influences tend to make urban history a large container with ill-defined, heterogeneous, and sometimes indiscriminate contents. It includes anything about cities. But it is clear that it raises problems peculiarly germane to social history, at least in the sense that the city can never be an analytical framework for economic macrohistory (because economically it must be part of a larger system), and politically it is only rarely found as a self-contained city state. It is essentially a body of human beings living together in a particular way, and the characteristic process of urbanization in modern societies makes it, at least up to the present, the form in which most of them live together. The technical, social, and political problems of the city arise essentially out of the interactions of masses of human beings living in close proximity to one another; and even the ideas about the city (insofar as it is not a mere stage-set for the display of some ruler's power and glory) are those in which men—from the Book of Revelation on—have tried to express their aspirations about human communities. Moreover, in recent centuries it has raised and dramatized the problems of rapid social change more than any other institution. That the social historians who have flocked into urban studies are aware of this need hardly be said.[11] One may say that they have been groping toward a view of urban history as a paradigm of social change. I doubt whether it can be this, at least for the period up to the present. I also doubt whether many really impressive global studies of the larger cities of the industrial era have so far been produced, considering the vast quantity of work in this field. However, urban history must remain a central concern of historians of society, if only because it brings out—or can bring out—those specific aspects

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of societal change and structure with which sociologists and social psychologists are peculiarly concerned.

The other clusters of concentration have not so far been institutionalized, though one or two may be approaching this stage of development. The history of classes and social groups has plainly developed out of the common assumption that no understanding of society is possible without an understanding of the major components of all societies no longer based primarily on kinship. In no field has the advance been more dramatic and—given the neglect of historians in the past—more necessary. The briefest list of the most significant works in social history must include Lawrence Stone on the Elizabethan aristocracy, E. Le Roy Ladurie on the Languedoc peasants, Edward Thompson on the making of the English working class, Adeline Daumard on the Parisian bourgeoisie; but these are merely peaks in what is already a sizeable mountain range. Compared to these the study of more restricted social groups—professions, for instance—has been less significant.

The novelty of the enterprise has been its ambition. Classes, or specific relations of production such as slavery, are today being systematically considered on the scale of a society, or in intersocietal comparison, or as general types of social relationship. They are also now considered in depth, that is, in all aspects of their social existence, relations, and behavior. This is new, and the achievements are already striking, though the work has barely begun—if we except fields of specially intense activity, such as the comparative study of slavery. Nevertheless, a number of difficulties can be discerned, and a few words about them may not be out of place.

(1) The mass and variety of material for these studies is such that the preindustrial artisan technique of older historians is plainly inadequate. They require cooperative teamwork and the utilization of modern technical equipment. I would guess that the massive works of individual scholarship will mark the early phases of this kind of research, but will give way on the one hand to systematic cooperative projects (such as the projected study of the Stockholm working class in the nineteenth century)[12] and on the other hand to periodic (and probably still single-handed) attempts at synthesis. This is evident in the field of work with which I am most familiar, the history of the working class. Even the most ambitious single work—E. P. Thompson's—is no more than a great torso, though it deals with a rather short period. (Jürgen Kuczynski's

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titanic *Geschichte der Lage der Arbeiter unter dem Kapitalismus*, as its title implies, concentrates only on certain aspects of the working class.)

(2) The field raises daunting technical difficulties, even where conceptual clarity exists, especially as regards the measurement of change over time—for example, the flow into and out of any specified social group, or the changes in peasant landholdings. We may be lucky enough to have sources from which such changes can be derived (for example, the recorded genealogies of the aristocracy and gentry as a group) or from which the material for our analysis may be constructed (for example, by the methods of historical demography, or the data on which the valuable studies of the Chinese bureaucracy have been based). But what are we to do, say, about Indian castes, which we also know to have contained such movements, presumably intergenerational, but about which it is so far impossible to make even rough quantitative statements?

(3) More serious are the conceptual problems, which have not always been clearly confronted by historians—a fact which does not preclude good work (horses can be recognized and ridden by those who can't define them), but which suggests that we have been slow to face the more general problems of social structure and relations and their transformations. These in turn raise technical problems, such as those of the possibly changing specification of the membership of a class over time, which complicates quantitative study. It also raises the more general problem of the multidimensionality of social groups. To take a few examples, there is the well-known Marxian duality of the term "class." In one sense it is a general phenomenon of all post-tribal history, in another a product of modern bourgeois society; in one sense almost an analytical construct to make sense of otherwise inexplicable phenomena, in another a group of people actually seen as belonging together in their own or some other group's consciousness, or both. These problems of consciousness in turn raise the question of the language of class—the changing, often overlapping, and sometimes unrealistic terminologies of such contemporary classification[13] about which we know as yet very little in quantitative terms. (Here historians might look carefully at the methods and preoccupations of social anthropologists, while pursuing—as L. Girard and a Sorbonne team are doing—the systematic quantitative study of sociopolitical vocabulary.[14])

Again, there are degrees of class. To use Theodore Shanin's

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phrase,[15] the peasantry of Marx's 18th Brumaire is a "class of low classness," whereas Marx's proletariat is a class of very high, perhaps of maximal "classness." There are the problems of the homogeneity or heterogeneity of classes; or what may be much the same, of their definition in relation to other groups and their internal divisions and stratifications. In the most general sense, there is the problem of the relation between classifications, necessarily static at any given time, and the multiple and changing reality behind them.

(4) The most serious difficulty may well be the one which leads us directly toward the history of society as a whole. It arises from the fact that class defines not a group of people in isolation, but a system of relationships, both vertical and horizontal. Thus it is a relationship of difference (or similarity) and of distance, but also a qualitatively different relationship of social function, of exploitation, of dominance/subjection. Research on class must therefore involve the rest of society of which it is a part. Slaveowners cannot be understood without slaves, and without the nonslave sectors of society. It might be argued that for the self-definition of the nineteenth-century European middle classes the capacity to exercise power over people (whether through property, keeping servants, or even—via the patriarchal family structure—wives and children), and of not having direct power exercised over themselves, was essential. Class studies are therefore, unless confined to a deliberately restricted and partial aspect, analyses of society. The most impressive—like Le Roy Ladurie's—therefore go far beyond the limits of their title.

It may thus be suggested that in recent years the most direct approach to the history of society has come through the study of class in this wider sense.[16] Whether we believe that this reflects a correct perception of the nature of post-tribal societies, or whether we merely put it down to the current influence of *Marxisant* history, the future prospects of this type of research appear bright.

In many ways the recent interest in the history of "mentalities" marks an even more direct approach to central methodological problems of social history. It has been largely stimulated by the traditional interest in "the common people" of many who are drawn to social history. It has dealt largely with the individually inarticulate, undocumented, and obscure, and is often indistinct from an interest in their social movements or in more general phenomena of social behavior, which today, fortunately, also includes an interest in

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those who fail to take part in such movements—for example, in the conservative as well as in the militant or passively socialist worker.

This very fact has encouraged a specifically dynamic treatment of culture by historians, superior to such studies as those of the "culture of poverty" by anthropologists, though not uninfluenced by their methods and pioneering experience. They have been not so much studies of an aggregate of beliefs and ideas, persistent or not —though there has been much valuable thought about these matters, for example, by Alphonse Dupront[17]—as of ideas in action and, more specifically, in situations of social tensions and crisis, as in Georges Lefebvre's *Grande Peur*, which has inspired so much subsequent work. The nature of sources for such study has rarely allowed the historian to confine himself to simple factual study and exposition. He has been obliged from the outset to construct models, that is, to fit his partial and scattered data into coherent systems, without which they would be little more than anecdotal. The criterion of such models is or ought to be that its components should fit together and provide a guide to both the nature of collective action in specifiable social situations and to its limits.[18] Edward Thompson's concept of the "moral economy" of preindustrial England may be one such; my own analysis of social banditry has tried to base itself on another.

Insofar as these systems of belief and action are, or imply, images of society as a whole (which may be, as occasion arises, images either seeking its permanence or its transformation), and insofar as these correspond to certain aspects of its actual reality, they bring us closer to the core of our task. Insofar as the most successful such analyses have dealt with traditional or customary societies, even though sometimes with such societies under the impact of social transformation, their scope has been more limited. For a period characterized by constant, rapid, and fundamental change, and by a complexity which puts society far beyond the individual's experience or even conceptual grasp, the models derivable from the history of culture have probably a diminishing contact with the social realities. They may not even any longer be very useful in constructing the pattern of aspiration of modern society ("what society ought to be like"). For the basic change brought about by the Industrial Revolution in the field of social thought has been to substitute a system of beliefs resting on unceasing *progress* toward aims which can be specified only as a *process*, for one resting on the assumption of permanent order, which can be described

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or illustrated in terms of some concrete social model, normally drawn from the past, real or imaginary. The cultures of the past measured their own society against such specific models; the cultures of the present can measure them only against possibilities. Still, the history of "mentalities" has been useful in introducing something analogous to the discipline of the social anthropologists into history, and its usefulness is very far from exhausted.

I think the profitability of the numerous studies of social conflict, ranging from riots to revolutions, requires more careful assessment. Why they should attract research today is obvious. That they always dramatize crucial aspects of social structure because they are here strained to the breaking point is not in doubt. Moreover, certain important problems cannot be studied at all except in and through such moments of eruption, which do not merely bring into the open so much that is normally latent, but also concentrate and magnify phenomena for the benefit of the student, while—not the least of their advantages—normally multiplying our documentation about them. To take a simple example: How much less would we know about the ideas of those who normally do not express themselves commonly or at all in writing but for the extraordinary explosion of articulateness which is so characteristic of revolutionary periods, and to which the mountains of pamphlets, letters, articles, and speeches, not to mention the mass of police reports, court depositions, and general inquiries bear witness? How fruitful the study of the great, and above all the well-documented, revolutions can be is shown by the historiography of the French Revolution, which has been studied longer and more intensively perhaps than any period of equal brevity, without visibly diminishing returns. It has been, and still remains, an almost perfect laboratory for the historian.[19]

The danger of this type of study lies in the temptation to isolate the phenomenon of overt crisis from the wider context of a society undergoing transformation. This danger may be particularly great when we launch into comparative studies, especially when moved by the desire to solve problems (such as how to make or stop revolutions), which is not a very fruitful approach in sociology or social history. What, say, riots have in common with one another (for example, "violence") may be trivial. It may even be illusory, insofar as we may be imposing an anachronistic criterion, legal, political, or otherwise, on the phenomena—something which historical students of criminality are learning to avoid. The same may or may not be true of revolutions. I am the last person to wish to discourage

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an interest in such matters, since I have spent a good deal of professional time on them. However, in studying them we ought to define the precise purpose of our interest clearly. If it lies in the major transformations of society, we may find, paradoxically, that the value of our study of the revolution itself is in inverse proportion to our concentration on the brief moment of conflict. There are things about the Russian Revolution, or about human history, which can only be discovered by concentrating on the period from March to November 1917 or the subsequent Civil War; but there are other matters which cannot emerge from such a concentrated study of brief periods of crisis, however dramatic and significant.

On the other hand, revolutions and similar subjects of study (including social movements) can normally be integrated into a wider field which does not merely lend itself to, but requires, a comprehensive grasp of social structure and dynamics: the short-term social transformations experienced and labeled as such, which stretch over a period of a few decades or generations. We are dealing not simply with chronological chunks carved out of a continuum of growth or development, but with relatively brief historic periods during which society is reoriented and transformed, as the very phrase "industrial revolution" implies. (Such periods may of course include great political revolutions, but cannot be chronologically delimited by them.) The popularity of such historically crude terms as "modernization" or "industrialization" indicates a certain apprehension of such phenomena.

The difficulties of such an enterprise are enormous, which is perhaps why there are as yet no adequate studies of the eighteenth-nineteenth century industrial revolutions as social processes for any country, though one or two excellent regional and local works are now available, such as Rudolf Braun on the Zurich countryside and John Foster on early nineteenth-century Oldham.[20] It may be that a practicable approach to such phenomena can be at present derived not only from economic history (which has inspired studies of industrial revolution), but from political science. Workers in the field of the prehistory and history of colonial liberation have naturally been forced to confront such problems, though perhaps in an excessively political perspective, and African studies have proved particularly fruitful, though recent attempts to extend this approach to India may be noted.[21] In consequence the political science and political sociology dealing with the modernization of colonial societies can furnish us with some useful help.

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The analytical advantage of the colonial situation (by which I mean that of *formal* colonies acquired by conquest and directly administered) is that here an entire society or group of societies is sharply defined by contrast with an outside force, and its various internal shifts and changes, as well as its reactions to the uncontrollable and rapid impact of this force, can be observed and analyzed as a whole. Certain forces which in other societies are internal, or operate in a gradual and complex interaction with internal elements of that society, can here be considered for practical purposes and in the short run as entirely external, which is analytically very helpful. (We shall not of course overlook the distortions of the colonial societies—for example, by the truncation of their economy and social hierarchy—which also result from colonization, but the interest of the colonial situation does not depend on the assumption that colonial society is a replica of noncolonial.)

There is perhaps a more specific advantage. A central preoccupation of workers in this field has been nationalism and nation-building, and here the colonial situation can provide a much closer approximation to the general model. Though historians have hardly yet come to grips with it, the complex of phenomena which can be called national(ist) is clearly crucial to the understanding of social structure and dynamics in the industrial era, and some of the more interesting work in political sociology has come to recognize it. The project conducted by Stein Rokkan, Eric Allardt, and others on "Centre Formation, Nation-Building and Cultural Diversity" provides some very interesting approaches.[22]

The "nation," a historical invention of the past two hundred years, whose immense practical significance today hardly needs discussion, raises several crucial questions of the history of society, for example, the change in the scale of societies, the transformation of pluralist, indirectly linked social systems into unitary ones with direct linkages (or the fusion of several preexisting smaller societies into a larger social system), the factors determining the boundaries of a social system (such as territorial-political), and others of equal significance. To what extent are these boundaries objectively imposed by the requirements of economic development, which necessitate as the locus of, for example, the nineteenth-century type industrial economy a territorial state of minimum or maximum size in given circumstances?[23] To what extent do these requirements automatically imply not only the weakening and destruction of earlier social structures, but also particular degrees of simplification,

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standardization, and centralization—that is, direct and increasingly exclusive links between "center" and "periphery" (or rather "top" and "bottom")? To what extent is the "nation" an attempt to fill the void left by the dismantling of earlier community and social structures by inventing something which could function as, or produce symbolic substitutes for, the functioning of a consciously apprehended community or society? (The concept of the "nation-state" might then combine these objective and subjective developments.)

The colonial and ex-colonial situations are not necessarily more suitable bases for investigating this complex of questions than is European history, but in the absence of serious work about it by the historians of nineteenth- and twentieth-century Europe, who have been hitherto—including the Marxists—rather baffled by it, it seems likely that recent Afro-Asian history may form the most convenient starting-point.

V

How far has the research of recent years advanced us toward a history of society? Let me put my cards on the table. I cannot point to any single work which exemplifies the history of society to which we ought, I believe, to aspire. Marc Bloch has given us in *La société féodale*, a masterly, indeed an exemplary, work on the nature of social structure, including both the consideration of a certain type of society and of its actual and possible variants, illuminated by the comparative method, into the dangers and the much greater rewards of which I do not propose to enter here. Marx has sketched out for us, or allows us to sketch for ourselves, a model of the typology and the long-term historical transformation and evolution of societies which remains immensely powerful and almost as far ahead of its time as were the Prolegomena of Ibn Khaldun, whose own model, based on the interaction of different types of societies, has of course also been fruitful, especially in pre-history, ancient, and oriental history. (I am thinking of the late Gordon Childe and Owen Lattimore.) Recently there have been important advances toward the study of certain types of society—notably those based on slavery in the Americas (the slave-societies of antiquity appear to be in recession) and those based on a large body of peasant cultivators. On the other hand the attempts to translate a comprehensive social history into popular synthesis strike me so far as

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either relatively unsuccessful or, with all their great merits, not the least of which is stimulation, as schematic and tentative. The history of society is still being constructed. I have in this essay tried to suggest some of its problems, to assess some of its practice, and incidentally to hint at certain problems which might benefit from more concentrated exploration. But it would be wrong to conclude without noting, and welcoming, the remarkably flourishing state of the field. It is a good moment to be a social historian. Even those of us who never set out to call ourselves by this name will not want to disclaim it today.

References

1. See the remarks of A. J. C. Rueter in *IX congrès international des sciences historiques* (Paris, 1950), I, 298.

2. R. H. Tawney, *Studies in Economic History* (London, 1927), pp. xxiii, 33-34, 39.

3. J. H. Clapham, *A Concise Economic History of Britain* (Cambridge, Eng.: University Press, 1949), introduction.

4. Two quotations from the same document (Economic and Social Studies Conference Board, *Social Aspects of Economic Development,* Istanbul, 1964) may illustrate the divergent motivations behind this new preoccupation. By the Turkish president of the board: "Economic development or growth in the economically retarded areas is one of the most important questions which confronts the world today . . . Poor countries have made of this issue of development a high ideal. Economic development is to them associated with political independence and a sense of sovereignty." By Daniel Lerner: "A decade of global experience with social change and economic development lies behind us. The decade has been fraught with efforts, in every part of the world, to induce economic development without producing cultural chaos, to accelerate economic growth without disrupting societal equilibrium; to promote economic mobility without subverting political stability" (xxiii, 1).

5. Sir John Hicks's complaint is characteristic: "My 'theory of history' . . . will be a good deal nearer to the kind of thing that was attempted by Marx . . . Most of [those who believe ideas can be used by historians to order their material, so that the general course of history can be fitted into place] . . . would use the Marxian categories, or some modified version of them; since there is so little in the way of an alternative version that is available, it is not surprising that they should. It does, nevertheless, remain extraordinary that one hundred years after *Das Kapital,* after a century during which there have been enormous developments in social science, so little else should have emerged." *A Theory of Economic History* (Oxford: Clarendon Press, 1969), pp. 2-3.

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6. Thus Marc Ferro's sampling of the telegrams and resolutions sent to Petrograd in the first weeks of the February revolution of 1917 is plainly the equivalent of a retrospective public opinion survey. One may doubt whether it would have been thought of without the earlier development of opinion research for nonhistorical purposes. M. Ferro, *La Révolution de 1917* (Paris: Aubier, 1967).

7. At the conference on New Trends in History, Princeton, N. J., May 1968.

8. I do not regard such devices for inserting direction into societies as "increasing complexity" as historical. They may, of course, be true.

9. P. Baran, *The Political Economy of Growth* (New York: Monthly Review Press, 1957), chap. 2.

10. For an English version of this important article, see *Social Science Information,* 9 (February 1970), 145-174.

11. Cf. "At stake in a broader view of urban history is the possibility of making the societal process of urbanization central to the study of social change. Efforts should be made to conceptualize urbanization in ways that actually represent social change." Eric Lampard in Oscar Handlin and John Burchard, *The Historians and the City* (Cambridge, Mass.: M.I.T. Press, 1963), p. 233.

12. This work is in progress under the direction of Professor Sven-Ulric Palme at the University of Stockholm.

13. For the possible divergences between reality and classification, see the discussions about the complex socioracial hierarchies of colonial Latin America. Magnus Mörner, "The History of Race Relations in Latin America," in L. Foner and E. D. Genovese, *Slavery in the New World* (Englewood Cliffs, N. J.: Prentice-Hall, 1969), p. 221.

14. See A. Prost, "Vocabulaire et typologie des familles politiques," *Cahiers de lexicologie,* XIV (1969).

15. T. Shanin, "The Peasantry as a Political Factor," *Sociological Review,* 14 (1966), 17.

16. Class has long been the central preoccupation of social historians. See, for example, A. J. C. Rueter in *IX congrès international des sciences historiques,* I, 298-299.

17. A. Dupront, "Problèmes et méthodes d'une histoire de la psychologie collective," *Annales: économies, sociétés, civilisations,* 16 (January–February 1961), 3-11.

18. By "fitting together" I mean establishing a systematic connection between different, and sometimes apparently unconnected, parts of the same syndrome—for example, the beliefs of the classic nineteenth-century liberal bourgeoisie in both individual liberty and a patriarchal family structure.

19. We look forward to the time when the Russian Revolution will provide historians with comparable opportunities for the twentieth century.

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20. R. Braun, *Industrialisierung und Volksleben* (Erlenbach-Zurich: Rentsch, 1960); *Sozialer und kultureller Wandel in einem ländlichen Industriegebiet . . . im 19. und 20. Jahrhundert* (Erlenbach-Zurich: Rentsch, 1965). J. O. Foster's thesis is being prepared for publication.

21. Eric Stokes, who is doing this, is conscious of applying the results of work in African history. E. Stokes, *Traditional Resistance Movements and Afro-Asian Nationalism: The Context of the 1857 Mutiny-Rebellion in India* (forthcoming).

22. *Centre Formation, Nation-Building and Cultural Diversity: Report on a Symposium Organized by UNESCO* (duplicated draft, n.d.). The symposium was held August 28–September 1, 1968.

23. Though capitalism has developed as a global system of economic interactions, in fact the real units of its development have been certain territorial-political units— British, French, German, U. S. economies—which may be due to historic accident but also (the question remains open) to the necessary role of the state in economic development, even in the era of the purest economic liberalism.

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LAWRENCE STONE

Prosopography

Origins

In the last forty years collective biography (as the modern historians call it), multiple career-line analysis (as the social scientists call it), or prosopography (as the ancient historians call it) has developed into one of the most valuable and most familiar techniques of the research historian. Prosopography[1] is the investigation of the common background characteristics of a group of actors in history by means of a collective study of their lives. The method employed is to establish a universe to be studied, and then to ask a set of uniform questions—about birth and death, marriage and family, social origins and inherited economic position, place of residence, education, amount and source of personal wealth, occupation, religion, experience of office, and so on. The various types of information about the individuals in the universe are then juxtaposed and combined, and are examined for significant variables. They are tested both for internal correlations and for correlations with other forms of behavior or action.

Prosopography is used as a tool with which to attack two of the most basic problems in history. The first concerns the roots of political action: the uncovering of the deeper interests that are thought to lie beneath the rhetoric of politics; the analysis of the social and economic affiliations of political groupings; the exposure of the workings of a political machine; and the identification of those who pull the levers. The second concerns social structure and social mobility: one set of problems involves analysis of the role in society, and especially the changes in that role over time, of specific (usually elite) status groups, holders of titles, members of professional associations, officeholders, occupational groups, or economic classes; another set is concerned with the determination of the degree of social

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mobility at certain levels by a study of the family origins, social and geographical, of recruits to a certain political status or occupational position, the significance of that position in a career, and the effect of holding that position upon the fortunes of the family; a third set struggles with the correlation of intellectual or religious movements with social, geographical, occupational, or other factors. Thus, in the eyes of its exponents, the purpose of prosopography is to make sense of political action, to help explain ideological or cultural change, to identify social reality, and to describe and analyze with precision the structure of society and the degree and the nature of the movements within it. Invented as a tool of political history, it is now being increasingly employed by the social historians.

The major contributors to the development of prosopography can be divided into two fairly distinct schools. Those of the elitist school have been concerned with small-group-dynamics, or the interaction, in terms of family, marriage, and economic ties, of a restricted number of individuals. The subjects of study have usually been power elites, such as Roman or United States senators or English M.P.'s or cabinet members, but the same process and model can be and have been applied to revolutionary leaders as well.[2] The technique employed is to make a meticulously detailed investigation into the genealogy, business interests, and political activities of the group, the relationships being displayed by means of detailed case studies, supported to only a secondary and relatively minor degree by statistical underpinnings. The purpose of such research is to demonstrate the cohesive strength of the group in question, bound together by common blood, background, education, and economic interests, to say nothing of prejudices, ideals, and ideology. When the main problem is political, it is argued that it is this web of purely social and economic ties which gave the group its unity and therefore its political force, and to a considerable extent also its political motivation, inasmuch as politics is a matter of the ins against the outs. This school has owed little or nothing to the social sciences, despite the fact that it could have learned much from them, and has been largely innocent of conscious sociological or psychological theory. Its assumptions, however, are clearly that politics is a matter of the interplay of small ruling elites and their clients rather than mass movements, and that self-interest, meaning a fierce Hobbesian competition for power and wealth and security, is what makes the world go round.[3]

The second is the more statistically-minded mass school, which

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deliberately draws its inspiration from the social sciences. The members of this school have mostly, but by no means entirely, concerned themselves with large numbers, about all—or indeed sometimes any—of whom in the nature of things nothing very detailed or very intimate can be known, since they are dead and therefore unavailable for an interview. The members of this school have a sense that history is determined by the movements of popular opinion rather than by the decisions of so-called "great men," or by elites, and they have been aware that human needs cannot usefully be defined exclusively in terms of power and wealth. They have necessarily been more concerned with social history than political history, and have therefore tried to ask a wider, if inevitably more superficial, set of questions than those usually posed by the members of the elitist school. They have also been far more concerned with testing the statistical correlations of the many variables than with conveying a sense of historical reality by a series of detailed case studies. Insofar as they have tried to describe the past they have tended to do so more by the construction of Weberian ideal-types than by presenting a series of concrete examples. Much of their work has been concerned with social mobility, but some of it has looked for statistically meaningful relationships between environment and ideas, and between ideas and political or religious behavior. The two schools therefore differ significantly in their subjects of study, and somewhat in their presuppositions, means, and end, but they are similar in their common interest in the group rather than in the individual or the institution.

Both the elitist and the mass schools first became clearly identifiable in the profession in the 1920's and 1930's, when a number of works appeared that had a profound effect on all subsequent development. The raw materials from which these prosopographical studies were and are constructed are mostly of three broad kinds: bare lists of names of holders of certain offices or titles, or professional or educational qualifications; family genealogies; and full biographical dictionaries, which are usually built up in part from the first two categories and in part from an infinitely wider range of sources. The collection of biographical materials of this kind had been in progress for a very long time before the first professional prosopographers appeared on the scene. To take the case of English history (although Roman history would be an equally good example),[4] throughout the late eighteenth, nineteenth, and early twentieth centuries, diligent antiquaries, clergymen, and scholars

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had been producing biographical information of all kinds in quite astonishing quantities. From public and private presses there poured a flood of biographical collections of every description and every quality: M.P.'s, peers, baronets, gentry, Archbishops of Canterbury, London clergy, lords chancellor, judges, sergeants at law, army officers, Catholic recusants, Huguenot refugees, Oxford and Cambridge alumni—the list is almost endless.[5]

The purpose of this outpouring—which was matched in the United States, Germany, and elsewhere—is not at all clear, since prosopography as a historical method had not been invented, and these publications were not used by professional historians except as quarries from which to dig out chunks of information about particular individuals. In terms of psychological motivation, these obsessive collectors of biographical information belong to the same category of anal-erotic males as the collectors of butterflies, postage stamps, or cigarette cards; all are by-products of the Protestant Ethic. But part of the stimulus came from local or institutional pride and affection, which took the form of a desire to record the past members of a corporation, college, profession, or sect. Part also derived from that inexhaustible passion for genealogy and ancestor-hunting which has gripped large sections of the English upper classes since the sixteenth century. With the huge expansion of the educated middle class in the nineteenth century, and with the growth of university and public libraries, there was at last a large enough market to justify the publication of these rather esoteric and unreadable volumes.

The supreme achievement of this century-long English movement for collective biography was the undertaking of the great *Dictionary of National Biography*, which is an enduring monument to the drive and dedication of the Victorians in the pursuit of information about the individual dead. When the first historical prosopographers got down to work after the First World War, they therefore found at hand a mass of biographical information already collected and in print, and merely waiting to be analyzed, collated, and used to construct an intelligible picture of society and politics.

The first historian to adopt the elitist method of prosopography to attack a major historical problem was Charles Beard, who as early as 1913 offered an explanation of the establishment of the American Federal Constitution by a close analysis of the economic and class interests of the Founding Fathers.[6] In the key chapter, "The Economic Interests of the Members of the Convention," he asked him-

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self whether they represented "distinct groups whose economic interests they understood and felt in concrete, definite forms through their own personal experience with identical property rights, or were they working merely under the guidance of abstract principles of political science." His conclusion was unambiguous: "The first firm steps towards the formation of the Constitution were taken by a small and active group of men immediately interested through their personal possessions in the outcome of their labors," a conclusion reached through an economic biography of all those connected with its framing. This remarkable and brilliant pioneering work seems to have had curiously little influence on postwar developments, perhaps because of the dogmatically rigid framework of economic determinism within which it was constructed. In his preface of 1935 Beard attempted to deny that his attitude to economic determinism was all embracing, that he was deeply influenced by Marxist thought, or that he was attributing sordid and self-interested motives to the Founding Fathers. But his disclaimers are not altogether convincing.[7] What Beard contributed to elitist prosopography was a suspicious curiosity about the finances of a political actor and the hypothesis that they are important. What he missed was the role of social and kinship ties which were to bulk so large in the later studies of Sir Lewis Namier and others. On the other hand, Beard's work should have been familiar to Namier, who, however much he may have been repelled by the Marxist economic determinism, must surely have been impressed by the interpretive power of the method.

A year later another American scholar, A. P. Newton, published a less well-known book which carried the method a little further.[8] He carefully tracked down kinship relationships and economic connections in order to demonstrate the formation of the Puritan opposition leadership to Charles I in the 1630's. His book was clearly a modest forerunner of Namier, but for some reason, perhaps because of the rather forbidding title, it never attracted much general attention.[9]

The real breakthrough into general acceptance by the profession did not come until the publication of Namier's *Structure of Politics at the Accession of George III* (London, 1929), Sir Ronald Syme's *Roman Revolution* (Oxford, 1939), and R. K. Merton's *Science, Technology, and Puritanism in Seventeenth Century England* (*Osiris*, IV, 1938). All three were able to draw on the store of biographical information which had been accumulated and published

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over the previous century. Merton used the *Dictionary of National Biography* for his work, Syme was indebted to two German historians, M. Gelzer and F. Münzer,[10] and Namier could exploit 130 years of data-collection about the lives of M.P's. The pioneer work of the prewar German school of historiography was of considerable importance for the later development of classical—and possibly also of modern—prosopography, but its achievements have been overshadowed by the more arresting and ambitious work of Namier and Syme. Apart from Beard and Newton, the two latter were the first historians of outstanding capacity to use this kind of approach to attempt a major reinterpretation of a critical political development which had been studied ad nauseam by more conventional historians over a very long period. Both worked impressionistically through case studies and personal vignettes, which they used to build up a picture of elitist personal interests, mainly kinship groupings, business affiliations, and a complicated web of favors given and received.

The third study, by R. K. Merton, was rather different in both its objectives and its method. As befitted an American sociologist rather than a British historian, what he produced was a statistically based group biography, rather than a group portrait pieced together from a series of case studies. The problem he set himself was also different, since he was not trying to account for specific political actions but for a state of mind, and was explaining a mental set not by family ties or economic interests but by ideological affiliations: he was attempting to link a favorable attitude toward natural science with allegiance to what he loosely described as Puritanism. On the other hand, his work was similar to that of Namier and Syme in that he was examining, although at a much shallower depth of research, the behavior of an elite rather than a mass.

Both Syme and Namier, but particularly the latter, were to have an enormous influence on the next generation of scholars in their fields of specialty. Some years ago a reviewer surveyed the recent and current work of historians of eighteenth-century British politics, and from the problems they set themselves and the methods they used for resolving them, concluded that they were all members of a single corporation: "Namier, Inc."[11] Today both the case-study and the statistical methods—and especially the latter—have spread to other fields and time periods, and are being applied on an ever-widening scale to every aspect of the historical process, at every time and in every place. The mass school now has a flourishing

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political subbranch called psephology, or the analysis of the voting behavior of the electorate; and the elitist school has spawned a more scientific subbranch, roll-call analysis of the legislature. Both these new special fields are absorbing increasing amounts of time, money, and attention from historians and political scientists.[12]

Intellectual Roots

That these developments occurred at the same period in the writings of scholars working entirely independently (Sir Ronald Syme assures me that he had not read Namier) proves that there is more to them than mere serendipity. Prosopography would not have flourished the way it did in the 1920's and 1930's had it not been for a crisis in the historical profession, which was already discernible to the more perceptive young men of the coming generation.[13] This crisis stemmed from the near-exhaustion of the great tradition of Western historical scholarship established in the nineteenth century. Based on a very close study of the archives of the state, its glories had been institutional, administrative, constitutional, and diplomatic history. But the major advances in these areas had all been made by the race of giants of the late Victorian and Edwardian periods, the outstanding figures for English history being C. W. Stubbs, T. F. Tout, F. W. Maitland, and S. R. Gardiner. In their search for new and more fruitful ways to understand the working of the institutions, some young historians just before and after the First World War began to turn from the close textual study of political theories and constitutional documents or the elucidation of bureaucratic machinery to an examination of the individuals concerned and the experiences to which they had been subjected. Exasperated by the windy pieties of a generation of historical interpreters of the framing of the American Constitution, Beard introduced his own book with the acid remark that "The Constitution was of human origin, immediately at least, and it is now discussed and applied by human beings who find themselves engaged in certain callings, occupations, professions and interests." In his challenging introductory statement a quarter of a century later, Syme also declared open war upon the elder generation of historians.[14] When dealing with the attitudes of Parliament toward the American colonies before the Revolution, Namier did not bother himself with the political theory of no taxation without representation. Instead he asked: "What acquaintance with the American Colonies

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had the house in which the Stamp Act was passed and repealed, and in which the Townshend Duties were enacted? How many of its Members had been to the American Colonies, had connections with them, or had an intimate knowledge of American affairs? Were any of them American born?"[15]

Following this example similar questions about who rather than what have been asked about such diverse questions in English historiography as Magna Carta, the House of Commons, riots, the civil service, and the Cabinet.[16] The unstated premise is that an understanding of who the actors were will go far toward explaining the workings of the institution to which they belonged, will reveal the true objectives behind the flow of political rhetoric, and will enable us better to understand their achievement, and more correctly to interpret the documents they produced.

The direction in which this attack on the conventional approach to political institutions and policies would develop was powerfully influenced by other important trends in the intellectual climate of the period, of which the first and most important was cultural relativism. Greater familiarity with foreign countries through travel combined with the growing volume of anthropological studies to reveal the extraordinary range of cultural patterns that have been adopted by different societies around the world. The educated public became uneasily aware that morals, laws, constitutions, religious beliefs, political attitudes, class structures, and sexual practices differ widely from one society to another, and this awareness in time led to a recognition that there are few universal norms of human behavior or social organization. The stress on environmental conditioning as the determining factor in creating this variety was all the greater because the 1920's and 1930's was a period when genetic explanations of cultural differences were not treated with the seriousness it now begins to appear that they may possibly deserve.[17] Social Darwinism, which was a powerful influence around the turn of the century, laid far more stress on nurture than nature. Moreover, the Freudian psychologists, who soon afterwards began to come into their own, also laid great stress upon the role of nurture, with particular emphasis on childhood and early sexual experience. It must be admitted, however, that Freudian psychology has not been much use to the historian, who is usually unable to penetrate the bedroom, the bathroom, or the nursery. If Freud is right, and if these are the places where the action is, there is not much the historian can do about it. The subsequent modification of Freudian

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ideas by Erik Erikson, according to which character formation continues through childhood and adolescence, and crystallizes in an "identity crisis" just before maturity, opens up new possibilities for the historian, who can sometimes discover a little about the thoughts and feelings of his subject in adolescence, even if he knows little or nothing about his infancy and early childhood. Up to now, however, Eriksonian psychology has been very little used by historians, and a far more important influence upon the profession has been behaviorist theories of challenge and response to environmental pressures.

The third influential element in the intellectual climate of the age was the decay of confidence in the integrity of politicians, and the decline of faith in the importance of constitutions. Much of this cynicism was generated by the political and moral disaster of the First World War, followed by the collapse of hopes of a better world order. Many people came to believe that this was the time when millions died and European civilization disintegrated, while politicians jockeyed for place and power by outbidding each other in the jingo rhetoric of hate. The result was the penetration into intellectual circles and into the upper classes of the ancient folklore of the poor, that all politicians are crooks. This was the muckraking era, in which the top was blown off the nineteenth century by books like Lytton Strachey's *Eminent Victorians* (1918) and Mathew Josephson's *The Robber Barons* (1934). Nor should it be forgotten that the events of the period did nothing to redress the balance; it was the era of Teapot Dome, Jimmy Thomas, and Stavisky. These popular assumptions and actual discoveries about the moral, and in particular the financial, laxity of politicians, led historians to think that if only one could get access to the private papers of past political actors, similar motives would be revealed as a driving force in history.

Apart from Fascism (which had little intellectual appeal), Marxism was the only powerful ideology of the period. Marxism gave many historians a somewhat naïve belief in economic determinism, which strongly reinforced these dark suspicions about human motivation. Beard thus declared that "the direct compelling motive" behind the framers of the American Constitution "was the economic advantages which the beneficiaries expected would accrue to themselves first from their action." [18] In its early stages, therefore, prosopography reflected a deeply pessimistic attitude toward human affairs, and was conducted either by radicals under Marxist influence,

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like Beard, or by men like Sir Lewis Namier and Sir Ronald Syme, who are ostensibly of a conservative frame of mind. Syme frankly admitted of his own work that "The design has imposed a pessimistic and truculent tone, to the almost complete exclusion of the gentler emotions and the domestic virtues," while an early reviewer commented with dismay of Namier's book that "The political system which it describes is certainly not attractive, based as it was upon a possibly enlightened but a certainly sordid self-interest." [19]

Nor was this cynicism confined to attitudes toward individual politicians; it also covered political systems. If revolutions mean no more than substitution of one grasping and self-centered ruling elite for another, if a handful of unscrupulous men steer the ship of state the way they want, whatever the constitutional flag under which they sail, then the difference between tyranny and democracy becomes blurred, to say the least. From this point of view the elitist school of historical prosopographers of the 1930's was deeply affected by the contemporary crisis of confidence in democracy. Namier deliberately set out to destroy theories about a tyrannical conspiracy by George III against the British constitution, and Syme appeared to remove any basis for moral judgments about the destruction of the Roman republic by Augustus. In 1939 A. Momigliano applied to Syme his own description of Tacitus: "a monarchist from perspicacious despair of human nature." [20] Robert Dahl has rightly observed, however, that "for individuals with a strong strain of frustrated idealism, it [elite theory] has just the right touch of hard-boiled cynicism." [21] The elite theorist and the elite historian tend to be disappointed egalitarians, whose misanthropy springs directly from outraged moral sentiment.

The attitude toward the workings of politics taken by the early prosopographers appears to owe little to the writings of political theorists. Marx himself stressed the role first of the feudal lords and then of the bourgeoisie, and directed attention to the self-interest that guided their actions. But the first fully-fledged elitist political theories came out of Europe in the early twentieth century, with the writings of R. Michels, G. Mosca, and V. Pareto. Although Michels was available in French, Pareto and Mosca were not translated into English before the 1930's, and there is no evidence that they had the slightest influence in historical circles in the Anglo-Saxon world until that time. Namier, Merton, and Syme were strongly anti-Marxist, and yet only Merton appears to have been familiar with these non-Marxist elitist models. What we have, there-

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fore, is the development by political scientists of a full-blown theory of rule by elites a generation before the historians set to work. But apart from Merton the historians carried out their empirical studies based on their own semiconscious assumptions about political behavior, without the benefit of the political theory which would have provided them with the framework they needed. It is one of the more bizarre episodes of intellectual history, a consequence of the slowness of the great European social scientists to be translated into English, and of the isolation of history from the other social sciences in the early twentieth century.

A key feature of the elitist interpretation of the historical process is the deliberate and systematic removal of both party programs and ideological passions from the center of the political stage, and their replacement by a complex web uniting patrons with their clients and dependents. For Roman history, this is expressly stated by Professors L. R. Taylor and E. Badian.[22] For English history Namier substituted the "connection" for the party as the central organizing principle of mid-eighteenth-century politics, K. B. McFarlane invented the phrase "Bastard Feudalism" to represent not dissimilar patron-client relationships which he believed could explain the fifteenth century, while Sir John Neale borrowed the word "clientage" from the classical historians to make sense of the Elizabethan political system. In a key passage the latter wrote, "most of the gentry seem to have grouped themselves in close or loose relationships around one or other of the few great men of the country . . . The grouping and interdependence of the gentry, with its accompanying and constant struggle for prestige and supremacy, permeated English life. It assumed the part played by politics in our modern society, and in the country, is the main clue to parliamentary elections." [23] For some scholars, prosopography was not merely a way of ignoring passions and ideas, it was adopted for the specific purpose of neutralizing these disturbing and intractable elements.

A fourth stimulus to elitist prosopography, which in turn reinforced the new awareness of the essential role played in politics by associations of dependents, was the almost obsessive concern of the anthropologists for the family and kinship, the full impact of which is only just beginning to make itself fully felt in the historical profession today. It was Namier's work on mid-eighteenth-century English politics which first drew the attention of historians to the potentialities of family arrangements and kinship links as political

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bonds.[24] It is perhaps not too farfetched to see a parallel between the preoccupation with such linkages of the elitist school of historians and similar preoccupations in contemporary fiction, notably Proust's *A la recherche du temps perdu* and Antony Powell's more recent *Music of Time*.

These intellectual trends are sufficient in themselves to explain the rise of the elitist school between the wars. The more scientifically-orientated mass school obviously owed something to all of them, but much more to the concurrent rise of the social sciences. From Weber to Merton the most intelligent and most successful of the social scientists have limited themselves to advancing middle-range hypotheses about such things as suicide or bureaucracy or receptivity to right-wing political views. Historical prosopography is obviously immensely valuable as a source of material for such investigations, and it is no coincidence that Marx and Weber and Merton have all had strong historical interests. The main inspiration for the type of questions asked and the methods employed to solve them by Merton and by a host of subsequent historical investigators of the mass school was the development of social survey techniques. From them comes the confidence in the sampling method and the habit of asking a very wide range of questions, many of which turn out to be wholly irrelevant, in the hope of picking out the significant variables by statistical manipulation later on.

Given these many converging trends in the intellectual life of the period between the two World Wars, it is hardly surprising that it was then that prosopography grew up. Indeed in retrospect what is surprising is rather the slowness of its advance upon the historical stage, for it was not until the 1950's or even the 1960's that significant numbers of students began to use the method, and that a steady stream of useful findings began to be published.

Limitations and Dangers

Sufficient experience has now accumulated to make it possible to appreciate both the potentialities and the limitations of prosopographical studies. Some of the errors and deficiencies are inevitable consequences of pioneering in a new method, and can be avoided in the future by learning from the mistakes of the past. Others, however, go deeper, and arise from some political and psychological presuppositions which are embedded in the foundations upon which prosopography rests.

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Deficiencies in the Data

It is self-evident that biographical studies of substantial numbers of persons are possible only for fairly well-documented groups, and that prosopography is therefore severely limited by the quantity and quality of the data accumulated about the past. In any historical group, it is likely that almost everything will be known about some members of it, and almost nothing at all about others; certain items will be lacking for some, and different items will be lacking for others. If the unknowns bulk very large, and if with the seriously incompletes they form a substantial majority of the whole, generalizations based on statistical averages become very shaky indeed, if not altogether impossible. Studies which have to be confined to that tenth or twentieth part of the group about which enough is known depend for their reliability on the recorded minority being a genuinely random sample of the whole. But this is an unlikely assumption, since the very fact that more than usual has been recorded about the lives and careers of a tiny minority indicates that they were somehow atypical. To a degree which cannot be measured, studies based on such fragmentary evidence will tend to exaggerate, and perhaps hopelessly to distort, the status, education, upward mobility, and so forth of the group under examination. For most social groups in most areas prosopography cannot usefully be employed before the explosion of record materials in the sixteenth century, caused by the invention of the printing press, the spread of literacy, and the growth of the bureaucratic, record-keeping nation-state.

The only exception to this generalization is when there exists a single detailed census-type survey, such as the Florentine *catasto* of 1427. These rare documents allow the historian to make a cross section through a society at a given moment, but they cannot answer any questions about change over time, since there is usually nothing before or after with which to compare them. They also need to be handled with care, since they may silently omit certain classes of persons, such as beggars, their categories may be vague or erratic, and their financial statistics are likely to underestimate the affluence of the rich relative to the poor.

The second limitation imposed by the record evidence is that of status. At all times and in all places, the lower one goes in the social system the poorer becomes the documentation. As a result, most studies that have already been made or are in progress today

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have been devoted to elites. The most popular subject for prosopography has been and still is political elites, but other groups which lend themselves most readily to such treatment are members of certain high status categories, such as civil servants, army officers, upper clergy, intellectuals and educators, lawyers, doctors, members of other professional bodies, and industrial and commercial entrepreneurs. The only elements of the lower classes about whom something can be done in anything more than a highly impressionistic way are persecuted minorities, since police reports and legal records often supply much of the necessary information, especially in societies with a long tradition of heavy bureaucratic and police control like France. The odd result is that the only groups of poor and humble about whom we can sometimes find out a good deal are minority groups, which are by definition exceptional since they are in revolt against the *mores* and beliefs of the majority.

The third limitation imposed by the evidence arises from the fact that it is abundant for some aspect of human life and almost nonexistent for others. The surviving records are concerned first and foremost with the amount, type, ownership, and transmission of property. It is this which is the prime concern of official and private legal records, official tax records, and public and private administrative records, which together form the vast bulk of the written material of the past. There is thus a strong bias toward treating the individual as *homoeconomicus,* and to study him primarily in the light of his financial interests and behavior, since this is what the records illuminate in the greatest clarity and detail. But economic interests may conflict, and even when the interest is clear, it is impossible to be sure that this is the overriding consideration. Moreover the split between the compromisers and the last-ditchers is often more important politically than the split between clearly defined economic interest-groups.[25]

After economic interests, the second item of information that is relatively easy to discover about a person is his family background and connections. Among the upper classes marriage has been used in the past to provide young men with useful friends and contacts, as well as to merge properties and so create great territorial estates. Family ties have also played an important part in the construction of political groups and parties at all times from the Middle Ages to the eighteenth century and beyond. One has only to think of the Howards and Dudleys in sixteenth-century England, the Villiers in the early seventeenth century, the Pelhams in the eighteenth, and

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the Cecils and Cavendishes in the late nineteenth and early twentieth, to recognize the continuing importance of this factor. But this does not answer the question of how far it is safe to pursue this line of reasoning, for the cementing role of kinship clearly varies from place to place, from time to time, and from social level to social level. There are countless examples in history of members of the same family who have disagreed among themselves, often with extreme violence. Moreover, even when kinship ties were strong and can be shown to have been so, there are limits to the meaningful pursuit of genealogical links. Two diligent prosopographers working on the Long Parliament of 1640 tracked down genealogical connections which related the radical John Hampden to eighty fellow M.P.'s, but unfortunately these kin turned out to be of widely varying political and religious opinions. When the authors found that by going back far enough they could find a kinship connection between Charles I and Oliver Cromwell, they realized that they had perhaps passed beyond the outer limits of utility of this particular line of inquiry. Similar doubts have been recently expressed about the role ascribed by the prosopographical school to kinship in classical Rome.[26]

Errors in the Classification of the Data

Meaningful classification is essential to the success of any study, but unfortunately for the historian every individual plays many roles, some of which are in conflict with others. He belongs to a civilization, to a national culture, and to a host of subcultures—ethnic, professional, religious, peer-group, political, social, occupational, economic, sexual, and so on. As a result, no one classification is of universal validity, and a perfect congruence of classifications is quite rare. Status categories may bear little relation to wealth, and also may vary in their importance over time. Class categories based on wealth may not reflect social realities, may be almost impossible to identify, and may be even more impossible to compare over time; professional categories may cut through both status and class lines and run vertically up and down the social system; power categories, such as political offices, may vary over time in the social status attached to them, in the power they wield, and in the income they produce.

The second danger which threatens every prosopographer is that he may fail to identify important subdivisions, and may thus be

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lumping together individuals who differ significantly from one another.[27] Good research depends on a constant interplay between the hypothesis and the evidence, the former undergoing repeated modification in the light of the latter. But if a subdivision which later turns out to be of critical importance has not been noted at the time, it is usually too late to go back and do the work all over again, a difficulty which is particularly acute in computer-aided studies, since the code book determines the questions that can later be asked.[28]

Errors in the Interpretation of the Data

Even if his documentation is adequate and his system of classification is properly designed, the unwary prosopographer is still liable to draw erroneous conclusions from his data. One common hazard which faces him is the possibility that that portion of the total population about which he can discover reliable information does not represent a random sample of the whole. If the unknowns mostly fall into a single slanted category, the figures taken from the sample of the known will give a distorted picture of reality. Thus Theodore Rabb himself provides reason to think that his sample of seventeenth-century English investors is biased, since it is quite likely that most of the unidentified 38 per cent of investors, both named and unnamed, were petty merchants.[29] This is a problem which affects all work which used this methodology, and against which the only defense is the most careful assessment of the probabilities, and the application where necessary of an index error to correct the statistics. Another mistake which often occurs in prosopographical studies springs from a failure to relate the findings about the composition of the group under study to that of the population at large. A good example of the difficulties into which the historian may stumble if he neglects this point is the dispute about the social composition of the victims of the Terror in the French Revolution. Professor D. Greer discovered that the great majority of the victims were drawn from the lower or middle class rather than the nobility. It has since been pointed out that the proportion of noble victims may have been very small, but since the proportion of nobles in the population at large was even smaller, there is still a correlation of noble birth with execution. One can still say that a nobleman had "X" times more chances of being killed in the Terror than a member of the bourgeoisie or a peasant.[30]

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Another type of error which arises from neglecting the relationship between the part and the whole springs from the assumption that because a majority of members of a certain group comes from a particular social class or occupation, that therefore they are representative, in the sense that a majority of members of their class or occupation belongs to the group. Hugh R. Trevor-Roper pointed out that the men who seized power in England in the late 1640's and early 1650's were mostly drawn not from the old landed elite who had ruled England before the war but from the poor gentry, mere gentry, or parish gentry, who had hitherto played no significant part in national and only a minor role in local affairs. Inspired by this discovery, he proceeded to generalize that the downwardly mobile mere gentry were the principle dissatisfied elements in the country and the main supporters of radicalism. In fact, however, it now seems fairly clear that a far larger number of the mere gentry—indeed the majority in the heartland of the class in the north and the west—were loyal church-and-king men who fought for King Charles. The Independent gentry who supported Cromwell were merely an untypical minority, goaded into taking a position so much at variance with that of most of their class by motives which at present we can only very dimly perceive, but one of which was certainly religious conviction.[31]

Limitations of Historical Understanding

So far, the errors which have been discovered have all been ones which can be avoided by learning the harsh lessons of experience, but there are others which will be more difficult to eradicate. In the first place, the concentration upon the study of elites has been part cause and part effect of a tendency to see history exclusively as a story of the ruling class, in which popular movements play little or no part. Syme claimed that "In all ages, whatever the form and name of government, be it monarchy, republic, or democracy, an oligarchy lurks behind the facade."[32] This is true enough as far as it goes, but one may reasonably question whether it goes far enough. Close study of the political maneuverings of the elite may conceal rather than illuminate the profounder workings of the social process. Major changes in class relationships, social mobility, religious opinions, and moral attitudes may be occurring among the lower strata, changes to which the elite will eventually be obliged to respond, if it is not to be swept away in violent revolution.[33]

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If we look at the three most brilliant examples of prosopographical research on political elites, Syme's *Roman Revolution*, Namier's *Structure of Politics*, and Sir John Neale's great trilogy on the Elizabethan House of Commons, published in the 1950's, we can see the same narrowing of focus. Syme interpreted the transformations of the Roman republic into an empire as the consolidation of a new elite around Augustus, the result of a complex factional in-fighting at the top. He proved his point, but he ignored the urgent demands of the nameless client masses upon their patrons which supported—and perhaps dictated—this shift of power. Political movements, and revolutions or counterrevolutions in particular, can hardly be satisfactorily explained by exclusive study of the leadership. Namier's picture of wheeling and dealing in the eighteenth-century House of Commons shattered conventional theories beyond repair, but his explanatory model could not include the springs of popular feeling generated by John Wilkes or the American War of Independence. Similarly, Sir John Neale's description of the relations between Queen Elizabeth and her Parliaments needs modification through greater appreciation of the deep roots that Puritanism was sinking into the society. This was an ideology which both cut across and exploited the nexus of aristocratic clientage which Sir John so brilliantly and convincingly described.

The second great intellectual weakness of prosopographers has been their relative unwillingness to build into their perspective of history a role for ideas, prejudices, passions, ideologies, ideals, or principles. Intimate personal correspondence is a rarity among historical records. It usually got destroyed during life or at death, since, unlike genealogical, legal, or business records, nobody among the family or friends had any incentive to preserve it. Even in the rare cases when such material exists, it is often not very illuminating, since men rarely commit their deepest convictions to paper, even with their friends. Moreover, since in most periods in history it has been positively dangerous to express minority views about religion or politics, such written comments as survive about basic issues tend to be confined within the accepted norms of the society. The systematic bias in the historical record in favor of material interests and kinship ties and against ideas and principles fitted in well with the explicitly stated presupposition of the greatest of the early elitist scholars.[31] "Spiritual interests of people are considered much less than their marriages," complained Momigliano as soon as Syme's book appeared. Sir Herbert Butterfield protested with ref-

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erence to Namier that "human beings are the carriers of ideas, as well as the repositories of vested interests."[35]

Despite some later disclaimers, there can be little doubt that in practice both Namier and Syme attached little importance to any ideal or prejudice which ran counter to the calculations of self-interest. The attention paid by these historians to the tactics rather than the strategy of politics presupposes a society without conviction in which manipulation and wire-pulling are more important than issues of principle or policy. It so happened that the mid-eighteenth century, upon which Namier first focused his attention, was a period in English history unusually devoid of major issues of controversy, and a period when the political actors formed an unusually homogeneous group: he thus chose, by accident or design, a period and a class which were especially susceptible to analysis by the methods he adopted. But some of his followers have found, to their cost, that it is not always safe to carry the same assumptions forwards and backwards in time. Robert Walcott tried to use the model for the reign of Queen Anne, with results that are now generally recognized to have been little short of disastrous.[36] One may also wonder whether Oliver Cromwell's failure successfully to manage his Parliaments can really be explained by his lack of tactical skill, as Professor Trevor-Roper argues, or whether disagreement over fundamental constitutional and religious issues between the military and the civilians, and between Independents, Presbyterians, and Anglicans, put a settlement quite out of reach of even the most shrewd and assiduous manipulator of men.[37] One may therefore conclude that the explanatory power of the interest-group theory of politics, which has tended to be associated with the elitist prosopographical approach, is much greater at some periods and in some places than it is at and in others. The fewer the major political issues, the lower the ideological temperature, the more oligarchic the political organization, the more likely it is to provide a convincing historical interpretation.

Another limitation of the prosopographical school of historians is that its members sometimes unduly neglect the stuff of politics, the institutional framework within which the system functions, and the narrative of how political actors shape public policy. "We are given a story that becomes silent or curiously neglectful as it touches the very things that government and Parliaments exist to do," complained Sir Herbert Butterfield. He concluded harshly that: "There is little interest in the work of ministers within their departments; in

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the springs of policy and the origins of important decisions; in the actual content of the political controversies of the time; in the attitude of the public to measures and men; and in the thrust and counter-thrust of parliamentary debate . . . Such tendencies are calculated to raise the question whether the new form of structural analysis is not capable of producing in the practitioners of the craft its own kind of occupational disease."[38] The disease of which Sir Herbert complains is a form of colorblindness which prevents its victims perceiving the political content of politics.

Many elitist prosopographers instinctively opt for a simplistic view of human motivation, according to which the springs of action are either one thing or another. We all of us ask our students to distinguish religious from political motives in the foreign policies of Gustavus Adolphus, or Oliver Cromwell, or whomever. In real life, human nature does not seem to function this way. The individual is moved by a convergence of constantly shifting forces, a cluster of influences such as kinship, friendship, economic interest, class prejudice, political principle, religious conviction, and so on, which all play their varying parts and which can usefully be disentangled only for analytical purposes. Moreover there is reason to think that the relative importance of the various background characteristics will vary from culture to culture and nation to nation and time to time; that some attitudes can be more closely related to identifiable background characteristics than others; and that some background characteristics are moderately influential over a large range of attitudes while others are highly influential over a single attitude.[39]

In any case it is essential to distinguish sharply between relatively minor matters over which a politician is ready enough to favor a relative or client or to receive a bribe, and major issues of principle, over which he is likely to follow the dictates of his head and his heart rather than those of his blood or his pocketbook.

Achievements

Nothing which has so far been said should be interpreted to mean that elitist prosopography is by its nature either useless or misleading. Red flags have been put up around the main danger spots where lie the bones of many pioneers in the method, and a case has been made for reducing the claims of prosopography generally as an explanatory tool. If past errors can be avoided, and if the limitations of the method are recognized, the potentialities are very great. In-

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deed, provided that it is accepted—as it surely must be—that values and behavior patterns are strongly influenced by past experience and upbringing, the power of the method can hardly be denied. All that is needed is more willingness to recognize the baffling complexity of human nature, the power of ideas, and the persistent influence of institutional structures. Prosopography does not have all the answers, but it is ideally fitted to reveal the web of sociopsychological ties that bind a group together. For example, to identify such ties among the leaders of the parliamentary opposition to Charles I in the late 1630's and early 1640's does not help us to decide whether economic or constitutional or religious issues caused the Civil War. But it does most powerfully illuminate the process of radical party formation, and in the end makes any such question seem redundant, for the simple reason that men do not tear up their political institutions by the roots unless all these influences are working together to form an overwhelming incentive for change.

The best way to illustrate the full range of the contribution which prosopography has made to historical understanding in the last twenty years is to focus on some particular time and place, for which the religious, social and political history of England between 1500 and 1660 will serve as well as any. The first major problem which has been enormously enriched by these studies is the English Reformation. Although during the 1950's and 1960's the dominant textbook interpreted this event in primarily political terms, as an act of state carried out by a handful of determined men at the top, there was at the same time in train a whole series of monographs which were to shatter this simple picture. Examination of the educational, moral, and financial condition of the pre-Reformation clergy has shown up their many shortcomings, but has also indicated that what was happening was not so much a decline in the quality and zeal of the clergy as a rise in the demands made upon them by the laity.[40] Viewed in this light, the Reformation becomes yet another "revolution of rising expectations." The monks have also been studied prosopographically, with similar results, and it has been established that there was a decline in numbers in the pre-Reformation period, and a massive voluntary flight from monastic seclusion in the early 1530's. Monasteries and nunneries can be seen trying desperately to adapt themselves to the needs of the upper class lay society by serving as old age homes for pensioned retainers and servants, as hotels for traveling gentlemen and noblemen, and as institutions for the deposit of unwanted children.[41] The fate of the

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monks after the Dissolution was early subjected to prosopographical analysis, which proved beyond doubt the fallacy of the hoary legend of the sufferings of the dispossessed.[42] The behavior of the bishops during the Reformation crisis has been elucidated and the divisions of opinion convincingly related to different education training—in theology or law—and to different career lines—in the church or the state bureaucracy.[43]

Even more important in its historical consequences than these valuable studies of members of the official hierarchies within the church has been the uncovering of the roots of religious radicalism in secular society. The great advance here came with the publication of Professor A. G. Dickens' pioneering work on *Lollards and Protestants in the Diocese of York* (1959), which used hitherto unexplored sources, and raised a whole range of new problems, which have since been further worked on by students and followers. Thanks to the patient tracking of Protestant heretics through secular and religious court records of prosecutions, the size, influence, social composition, occupational characteristics, and geographical spread of these persecuted minority groups have at last begun to emerge from the shadows. No serious scholar any longer dismisses the survival of Lollardy as of no consequence in the spread of radical religious ideas, and we can now see the dissemination of Protestant ideas not merely through the activities of a handful of scholars at Cambridge, but also through the penetration of imported Lutheran pamphlets, translated Bibles, and other subversive literature from the seaports to the inland areas via traders, cloth workers, dissident friars, and the like.[44]

The subsequent religious history of England has also benefited enormously from prosopography. The Marian exiles, who fled abroad to escape Catholic persecution between 1553 and 1558, have been shown to be an intellectual and social elite for which there is hardly any parallel before the flight of the Jews from Hitlerite Germany in the 1930's, and their role in determining the shape of the Anglican Settlement of 1558-1559 is now recognized to have been of the greatest importance.[45] Our understanding of why the Anglican church failed in its early years to win greater acceptance and to gain more converts has been illuminated through clerical prosopography, which has revealed the many shortcomings in numbers, education, zeal, and economic independence of the early Elizabethan parish clergy.[46] On one flank of the Established Church we are beginning to have a better picture of the growth of Puritanism through

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greater knowledge of who the Puritans were, though much work still remains to be done on Puritan merchants, dons, schoolmasters, clergy, and nobles.[47] On the other flank a very careful statistical and geographical comparison of Catholics in the 1560's and Catholics in the 1580's has proved conclusively, as no other method could, that the late Elizabethan development of Catholicism was a gentry-based revival stimulated by the missionary activities of the seminary priests, and not a survival of popular pre-Reformation Catholicism.[48]

Social history, which is concerned with groups rather than individuals, ideas, or institutions, is a field to which prosopography probably has most to contribute. Attempts to generalize about social change in advance of either detailed local studies or global statistics based on serious archival research lead to the kind of impasse into which the famous "gentry controversy" got itself stuck twenty years ago, during which rival hypotheses about broad social movements between 1540 and 1640 and their relationship to the revolution were bandied about on the basis of craftily selected examples whose typicality was altogether unknown. Since that time there have appeared several local studies of groups of gentry, and one general study on the aristocracy, which together go some way to eliminate certain hypotheses and to put statistical weight behind some others.[49]

For example, as a result of many years of very careful work upon the gentry of Yorkshire, it has been shown that of those gentry of the country who were in economic decline before the war and who took sides, three quarters joined the royalists and only one quarter the parliamentarians.[50] If this is true across the country, it disproves Professor Trevor-Roper's hypothesis that the radicals on the parliamentary side represented the declining "mere gentry." The same study also brings out the importance of Puritanism among so many parliamentarians and of Catholicism among a significant number of royalists. It adds one more nail to the coffin of the old Marxist theory, tentatively supported by R. H. Tawney and J. E. C. Hill, that the civil war was a conflict between capitalist entrepreneurial landlords and old-fashioned *rentiers*. In this case, detailed prosopographical analysis has put to the test—as nothing else could—the many theories of the social causes of the revolution, and has begun to sift truth from falsehood among them.[51]

As one might expect, the greatest concentration of prosopographical energy has been directed toward the political elite, and in particular toward the M.P.'s. The late nineteenth- and early twentieth-

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century historians had established the key role played in English political history by the increasingly independent and powerful House of Commons, and it had long been known that it was here that the basic issues were fought out. But it was not until after the Second World War that scholars began to ask what sort of people it was who made this history. Today we have studies of the M.P.'s of almost every Parliament between 1559 and 1660, and a much richer and more convincing picture has emerged as a result.[52] Through comparative statistics and a series of detailed case studies, we can watch the expansion in the numbers of M.P.'s and trace its cause in a desire by Elizabethan magnates to extend the range of their patronage, and in the willingness of Elizabeth to make concessions, however politically unwise in the long run, which did not cost her money in the short run. Statistical inquiries have revealed the striking growth in the educational training and administrative experience of M.P.'s and the persistent rise in the proportion of gentry. We know now how the members were elected and how electoral contests were fought and won, and we are beginning to learn a little about the changing relationship between the electors and their representatives. We can trace the decline in the electoral influence of the great court magnates before 1640, as it gave way to that of local gentlemen, and even of townsmen themselves for borough seats.

Prosopographical studies of local elites outside Parliament in the counties and the cities are just beginning to be even more helpful in illuminating the social and economic factors behind the party line-ups in the Civil War. They have already revealed that in some counties and towns—but not in all—there was a withdrawal from positions of authority in the late 1640's of members of the greater gentry and the old urban oligarchies, and their replacement by men drawn from the lesser gentry and small merchants, as more radical policies were adopted for the prosecution of the war and achievement of a political settlement.[53]

The principal conclusion which emerges from this survey of the literature is that the method works best when it is applied to easily defined and fairly small groups over a limited period of not much more than a hundred years, when the data is drawn from a very wide variety of sources which complement and enrich each other, and when the study is directed to solving a specific problem. Lollards and Protestants in the early sixteenth century, Captain Swing rioters in the early nineteenth, make ideal subjects. Ambitious sur-

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veys of many thousands of individuals over very long time-spans, using only the most easily accessible printed source materials, and applying a shotgun scatter approach to the problems which may be answered, are far less likely to produce worthwhile results.

Conclusion

Prosopography is today in the process of coming of age. It has passed through the follies and excesses of adolescence and is now settling down to the humdrum routine of responsible early middle age. If the elitist school had its origins in Germany and the United States, it was first developed in England, both in classical and in modern history, and a good deal of the best work still comes from there. But this early pioneering is now being overtaken, both in quantity and quality, by the scholarly outpourings from America. The latter has always been the main center of the mass school, the scale of whose output and the sophistication of whose methods is now increasing fast.[54] The principal causes of this proliferation of scientific historical prosopography in the United States has been the great influence of sociology and political science and the advanced training in the use of, and easy access to, the computer. The most impressive institutional achievement of this school has been the creation of the Inter-University Consortium for Political Research at the University of Michigan. Here there is being collected and put into machine-readable form information about the voting behavior, as recorded in congressional roll calls, of every congressman since 1789. In addition, the psephologists are being supplied with data about popular voting at the county level in every election since 1824, correlated with information from the census returns since 1790 about income, race, religion, and other key variables for each county and state.[55] A beginning is now being made in collecting machine-readable statistical data for earlier periods of American history and also for other countries.

It is indicative of the parting of the ways between British and American scholarship in the 1960's that the parallel monument to prosopography on the eastern side of the Atlantic takes the rather different form of the postwar History of Parliament project. Initiated and planned by Sir Lewis Namier, this began in 1951 and will result in a multi-volume biographical dictionary of all M.P.'s, linked by introductory volumes which use this personal information to provide illuminating case studies, to put together statistical comparisons, and

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to draw political conclusions. It is characteristic of the British approach that this project is paid for by the government and not by universities or foundations, that the biographical information it assembles is not being prepared in machine-readable form (except for one period under the editorship of an American), and that more emphasis is placed on the biographies and case studies than on the statistics.[56]

France is the third major center of historical research in the world, but for the last thirty years the best French historians have been preoccupied with some dazzlingly successful explorations of other new techniques of research. They have pioneered some brilliant environmental studies of local societies seen as a totality and examined in great depth, they have produced some massive statistical time-series about prices, foreign trade, and industrial output, and they have pioneered the scientific study of historical demography. Only in the last few years have French historians begun to take to prosopography, and in conformity with their long-standing emphasis upon quantification they are now embarked upon some very large-scale projects of the mass school, using the most sophisticated computer gadgetry.[57] These are being supported by the VI Section of the Ecole Pratique des Hautes Etudes in Paris, which for decades has been the center of statistical historical inquiry in France.

One of the reasons—although a poor and irrelevant one—why prosopography will continue to develop on both sides of the Atlantic is because it is so ideally suited to the requirements of research papers and doctoral dissertations. It introduces the novice student to a very wide range of sources, it teaches him to evaluate his evidence and to apply his judgment to resolve contradictions, it demands meticulous accuracy and the arrangement of information on a methodical basis, and it offers a topic which can readily be expanded or cut down by modifying the size of the sample in order to meet the requirements of available time and resources. Some of this research undoubtedly contributes to the New Antiquarianism—data collection for data collection's sake—but under skilled and organized leadership the projects can be fitted together by the director to produce a useful contribution to the sum of historical knowledge.

A second powerful—but equally irrelevant—reason for a further expansion of prosopography is the arrival of the computer, the full significance of which is only just becoming apparent. As historians slowly and timidly began to explore the potentialities of this new technological tool, they began to realize its almost limitless capacity

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for handling just the sort of material that prosopography throws up. The correlation of numerous variables affecting large masses of data, assembled on a uniform basis, is precisely what the computer can do best; it is also what is most laborious, and in many cases virtually impossible, for even the most mathematically-minded of historians working without electronic aids. It is painful to admit that the advent of a technical gadget should dictate the type of historical questions asked and the methods used for solving them, but it would be adopting the posture of the ostrich to pretend that this is not happening now, and will not happen on an even greater scale in the years to come.

It must be admitted that there are some serious dangers inherent in the very success and popularity of prosopography. The first is that the really large undertakings, like Sir John Neale's work on Elizabethan Parliaments, Professor W. K. Jordan's on charitable giving, or Sir Lewis Namier's even grander History of Parliament project, must be carried out by teams of researchers, assembling data on the lines laid down by the director. This material is then studied, collated, and eventually published by the director, to whom alone the credit goes.[58] Collective research is already fully accepted by the physical scientists as a familiar and necessary process, but it involves a degree of intellectual peonage by students and junior faculty to the professor, which many scholars bred in the older individualistic and independent tradition of historiography find disturbing. The second danger is that instead of coming together, the mass and the elitist schools will specialize more and more on their different approaches, the one becoming more scientific and quantitative and the other more impressionistic and devoted to individual examples inadequately controlled by random sampling. This would be a disaster for the profession, since it would spell the end of fruitful cross-fertilization. The danger has been greatly increased by the advent of the computer, which has been embraced by the more statistically-minded with all the undiscriminating enthusiasm of the nymphomaniac, and rejected by the less scientific partly from intellectual prudery, and partly from complacent ignorance of what pleasures they are missing. The availability of the computer will increasingly tempt some historians to concentrate their energies on problems that can be solved by quantification, problems which are sometimes—but by no means always—the most important or interesting ones. It will also tempt them to abandon sampling techniques, which are frequently perfectly adequate for their purposes, and to

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embark on very time-consuming statistical investigations of total populations, which in many cases is a wholly unnecessary procedure. Other historians may increasingly come to regard the computer as a threat to their intellectual predominance, and may retreat still further into the dark recesses of impressionistic methodology. To make matters worse, there are strong national overtones to the split, since the American and the French have far greater access to and confidence in the computer than their English colleagues, strong cultural overtones, with threats of a new war between the Ancients and the Moderns, the Humanities and the Sciences, and even philosophical overtones, with a clash between Fact and Fancy, Mr. Gradgrind and Sissy Jupe. As a result, it may be a long while before there is a full meeting of minds.

Prosopography nevertheless contains within it the potentiality to help in the re-creation of a unified field out of the loose confederation of jealously independent topics and techniques which at present constitutes the historian's empire. It could be a means to bind together constitutional and institutional history on the one hand and personal biography on the other, which are the two oldest and best developed of the historian's crafts, but which have hitherto run along more or less parallel lines. It could combine the humane skill in historical reconstruction through meticulous concentration on the significant detail and the particular example, with the statistical and theoretical preoccupations of the social scientists; it could form the missing connection between political history and social history, which at present are all too often treated in largely watertight compartments, either in different monographs or in different chapters of a single volume. It could help reconcile history to sociology and psychology. And it could form one string among many to tie the exciting developments in intellectual and cultural history down to the social, economic, and political bedrock. Whether or not prosopography will seize all or any of these opportunities will depend on the expertise, sophistication, modesty, and common sense of the next generation of historians.

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in proof). It provides a concise and accurate term for an increasingly common historical method, and is already in standard use by one group in the profession. It therefore seems very desirable that it should pass into everyday use among modern historians.

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6. Charles A. Beard, *An Economic Interpretation of the Constitution of the United States* (New York: Macmillan, 1913).

7. *Ibid.* (1935), pp. 73, 324, xii-xiv.

8. A. P. Newton, *The Colonising Activities of the English Puritans* (New Haven: Yale University Press, 1914).

9. It was not followed up until the publication of J. H. Hexter, *The Reign of King Pym* (Cambridge, Mass.: Harvard University Press, 1941).

10. M. Gelzer, *Die Nobilität der römischen Republik* (Leipzig-Berlin: B. G. Teubner, 1912); F. Munzer, *Römische Adelsparteien und Adelsfamilien* (Stuttgart, 1920).

11. John Raymond, *New Statesman* (October 19, 1957), pp. 499-500.

12. Some examples are published in D. K. Rowney and J. Q. Graham, *Quantitative History* (Homewood, Ill.: Dorsey Press, 1969), part VI.

13. The leaders of this intellectual revolution were the French, Marc Bloch and Lucien Febvre.

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14. Beard, *Economic Interpretation of the Constitution*, p. xiv; R. Syme, *The Roman Revolution* (Oxford: Oxford University Press, 1939), p. vii. For a description of this historiographical sea-change in Roman history, see Nicolet, "Prosopographie et histoire sociale," n. 4.

15. L. B. Namier, *England in the Age of the American Revolution*, 2d ed. (London: Macmillan, 1961), p. 229.

16. J. C. Holt, *The Northerners* (Oxford: Oxford University Press, 1961); J. E. Neale, *The Elizabethan House of Commons* (London: Cape, 1949); M. F. Keeler, *The Long Parliament, 1640-1641* (Philadelphia: American Philosophical Society, 1954); L. B. Namier and J. Brooke, *The House of Commons, 1754-1790* (London: Oxford University Press, 1964); E. J. Hobsbawm and G. Rudé, *Captain Swing* (London: Lawrence and Wishart, 1969); G. E. Aylmer, *The King's Servants: The Civil Service of Charles I, 1625-1642* (London: Routledge and Paul, 1961); W. L. Guttsman, *The British Political Elite* (London: Macgibbon and Kee, 1963).

17. For a suggestive, if highly speculative, survey of the possibilities of genetic influence see C. D. Darlington, "The Genetics of Society," *Past and Present*, 43 (1969).

18. Beard, *Economic Interpretation of the Constitution*, pp. 17-18.

19. Syme, *Roman Revolution*, p. viii; D. A. Winstanley, reviewing Namier in *English Historical Review*, 44 (1929), 660.

20. A. Momigliano reviewing Syme in *Journal of Roman Studies*, 30 (1940), 75.

21. As quoted in D. A. Rustow, "Study of Elites," p. 713.

22. L. R. Taylor, *Party Politics in the Age of Caesar* (Berkeley: University of California Press, 1949), p. 23; E. Badian, *Foreign Clientelae* (Oxford: Oxford University Press, 1958), p. 1.

23. K. B. McFarlane, "Bastard Feudalism," *Bulletin of the Institute for Historical Research*, 21 (1945); Neale, *Elizabethan House of Commons*, pp. 24, 27.

24. Namier, *England in the Age of the American Revolution*, p. 19. See also Syme, *Roman Revolution*, p. vii; Holt, *The Northerners;* Neale, *Elizabethan House of Commons;* N. Annan, "The Intellectual Aristocracy," in J. H. Plumb, ed., *Studies in Social History* (London: Longmans, Green, 1955).

25. For example, W. O. Aydelotte, "The Country Gentlemen and the Repeal of the Corn Laws," *English Historical Review*, 82 (1967); "Voting Patterns in the British House of Commons in the 1840's," in Rowney and Graham, *Quantitative History.*

26. D. Brunton and D. H. Pennington, *Members of the Long Parliament* (London: Allen and Unwin, 1954). For a convincing refutation of the theory "that genealogical and political links would normally coincide" in the early eighteenth century, see G. Holmes, *British Politics in the Age of Anne*

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(London: Macmillan, 1967), pp. 327-334. C. Meier, *Res Publica Amissa* (Wiesbaden: Steiner, 1966), and a review of it by P. A. Brunt in *Journal of Roman Studies,* 58 (1968), 229-232.

27. For an example which has been criticized on these grounds, see L. Stone, *The Crisis of the Aristocracy: 1558-1641* (Oxford: Oxford University Press, 1965). D. C. Coleman, "The 'Gentry' Controversy and the Aristocracy in Crisis, 1558-1641," *History,* 51 (1966); E. L. Petersen, "The Elizabethan Aristocracy Anatomized, Atomized and Reassessed," *Scandinavian Economic History Review,* 16 (1968); S. J. Woolf, "La Transformazione dell'Aristocrazia et la Revoluzione Inglese," *Studi Storici* (December 1968); J. H. Hexter, "The English Aristocracy, Its Crises, and the English Revolution, 1558-1660," *Journal of British Studies,* 8 (1968). The failure to work out sufficiently detailed subcategories seriously reduced the usefulness of Brunton and Pennington's study of the Long Parliament.

28. J.-Y. Tirat, "Problèmes de méthode en histoire sociale," *Revue d'Histoire Moderne et Contemporaine,* 10 (1963), 217.

29. T. K. Rabb, *Enterprise and Empire: Merchant and Gentry Investment in the Expansion of England, 1575-1630* (Cambridge, Mass.: Harvard University Press, 1967). For a review that makes this and other points, see J. J. McCusker in *Historical Methods Newsletter,* 2 (June 1969), 16-17. Another example of this problem is David Pottinger's claim that the writers of Old Regime France were drawn predominantly from the *noblesse d'épee* and the high bourgeoisie—a conclusion reached after the elimination of 48.5 per cent of all writers because their social background could not be discovered. D. Pottinger, *The French Book Trade in the Ancien Regime, 1500-1791* (Cambridge, Mass.: Harvard University Press, 1958). I owe this criticism to Professor Robert Darnton.

30. D. Greer, *The Incidence of the Terror During the French Revolution: A Statistical Interpretation,* 3d ed. (Cambridge, Mass.: Harvard University Press, 1964), pp. 385-387. A slightly different example of the same fallacy is D. Lerner's attempt to show that the Nazi leaders were "marginal men," when his definition of marginality clearly comprised over half the population (Rustow, "Study of Elites," p. 702).

31. H. R. Trevor-Roper, *The Gentry, 1540-1640* (*Economic History Review* Supplement I [1953]); W. G. Hoskins, "The Estates of the Caroline Gentry," in W. G. Hoskins and H. P. R. Finberg, eds., *Devonshire Studies* (London: Cape, 1952); J. T. Cliffe, *The Yorkshire Gentry* (London: Athlone Press, 1969), chap. 15; A. Everitt, *The Community of Kent and the Great Rebellion, 1640-1660* (Leicester: Leicester University Press, 1966), pp. 143-144, 243-244. For another example of the same error, see D. Donald, "Towards a Reconsideration of Abolitionists," in his *Lincoln Reconsidered* (New York: Knopf, 1956); R. A. Skotheim, "A Note on Historical Method: David Donald's Towards a Reconsideration of Abolitionists," *Journal of Southern History,* 25 (1959).

32. Syme, *Roman Revolution,* p. 7.

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33. See P. A. Brunt's remarks in *Journal of Roman History,* 58 (1968), 230-231.

34. Namier, *England in the Age of the American Revolution,* p. 18; Beard, *Economic Interpretation of the Constitution,* p. 13.

35. Momigliano reviewing Syme in *Journal of Roman Studies,* 30 (1940), p. 76; H. Butterfield, *George III and the Historians* (London: Collins, 1957), p. 211.

36. R. Walcott, *English Politics in the Early Eighteenth Century* (Oxford: Oxford University Press, 1956); J. H. Plumb, *The Origins of Political Stability: England, 1675-1725* (Boston: Houghton Mifflin, 1967), pp. xiv, 44-46, 135-138; Holmes, *British Politics in the Age of Anne,* pp. 2-4, 327-334.

37. H. R. Trevor-Roper, "Oliver Cromwell and His Parliament," in his *Religion, the Reformation and Social Change* (London: Macmillan, 1967).

38. Butterfield, *George III and the Historians,* pp. 208-209.

39. L. J. Edinger and D. S. Searing, "Social Background in Elite Analysis: A Methodological Enquiry," *American Political Science Review,* 61 (1967).

40. Peter Heath, *The English Parish Clergy on the Eve of the Reformation* (London: Routledge and Paul, 1969), pp. 187-196; M. Bowker, *The Secular Clergy in the Diocese of Lincoln, 1495-1520* (Cambridge, Eng.: Cambridge University Press, 1968).

41. G. A. J. Hodgett, "The Unpensioned Ex-Religious in Tudor England," *Journal of Ecclesiastical History,* 13 (1962).

42. G. Baskerville, *English Monks and the Suppression of the Monasteries* (London: Cape, 1937); Hodgett, "The Unpensioned Ex-Religious in Tudor England."

43. L. B. Smith, *Tudor Prelates and Politics* (Princeton: Princeton University Press, 1953).

44. M. Aston, "Lollardy and the Reformation: Survival or Revival?" *History,* 49 (1964); J. F. Davis, "Lollard Survival and the Textile Industry in the South-East of England," *Studies in Church History,* 3 (1966); W. Clebsch, *England's Earliest Protestants, 1520-1535* (New Haven: Yale University Press, 1964).

45. C. H. Garrett, *The Marian Exiles* (Cambridge, Eng.: Cambridge University Press, 1938); M. Walzer, *The Revolution of the Saints* (Cambridge, Mass.: Harvard University Press, 1965), pp. 92-113; J. E. Neale, *Elizabeth I and Her Parliaments, 1559-1581* (London: Cape, 1953), part I.

46. W. G. Hoskins, "The Leicestershire Country Parson in the Sixteenth Century," *Essays in Leicestershire History* (Liverpool: University Press, 1950); F. W. Brooks, "The Social Position of the Parson in the Sixteenth Century," *British Archaeological Society Journal,* 3d ser., 10 (1948); D. M. Barrett, "The Condition of the Parish Clergy Between the Reformation and

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1660," Ph.D. diss., Oxford, 1949; P. Tyler, "The Status of the Elizabethan Parochial Clergy," *Studies in Church History*, 4 (1957).

47. There is a good deal of incidental prosopographical material in P. Collinson's great book, *The Elizabethan Puritan Movement* (London: Cape, 1967). P. S. Seaver, *The Puritan Lectureships* (Stanford: Stanford University Press, 1970), chaps. 5, 6.

48. A. G. Dickens, "The First Stages of Romanist Recusancy in Yorkshire, 1560-1590," *Yorkshire Archaeological Journal*, 35 (1941). See also J. Bossy, "The Character of Elizabethan Catholicism," *Past and Present*, 21 (1962); B. Magee, *The English Recusants* (London: Burns, Oates, and Washbourne, 1938).

49. For a summary of the controversy see L. Stone, *Social Change and Revolution in England, 1540-1640* (London: Longmans, 1965), pp. xi-xxvi; M. E. Finch, *The Wealth of Five Northamptonshire Families, 1540-1640* (Oxford: Northamptonshire Record Society, 1956); Cliffe, *The Yorkshire Gentry;* H. A. Lloyd, *The Gentry of South-West Wales, 1540-1640* (Cardiff: University of Wales Press, 1968); Stone, *The Crisis of the Aristocracy*. In the last few years some twenty doctoral theses have been or are being written on groups of gentry in various counties.

50. Cliffe, *The Yorkshire Gentry*, p. 354. These percentages and the conclusions drawn from them are mine, not Dr. Cliffe's.

51. Prosopography has also undermined another hypothesis about the causes of the Civil War, namely H. R. Trevor-Roper's claims about the role of the bureaucracy. G. E. Aylmer, "Office-holding as a Factor in English History, 1625-42," *History*, 44 (1959).

52. Unpublished theses by pupils of Sir John Neale, a brilliant synthesis and interpretation of whose findings is set out in his *Elizabethan House of Commons*. T. L. Moir, *The Addled Parliament of 1614* (Oxford: Clarendon Press, 1958); Keeler, *The Long Parliament;* Brunton and Pennington, *Members of the Long Parliament;* P. J. Pinkney, "The Cromwellian Parliament of 1656," Ph.D. diss., Vanderbilt, 1962; M. E. W. Helms, "The Convention Parliament of 1660," Ph.D. diss., Bryn Mawr, 1963.

53. Everitt, *The Community of Kent*, p. 143; V. Pearl, *London and the Outbreak of the Puritan Revolution* (London: Oxford University Press, 1961), p. 160; R. G. Howell, *Newcastle upon Tyne and the Puritan Revolution* (Oxford: Clarendon Press, 1967), pp. 171-173. The old elite held on in Suffolk. See A. Everitt, *Suffolk and the Great Rebellion, 1640-1660*, Suffolk Record Society, 3 (1960).

54. Distinguished elite studies by American scholars in American history include: J. T. Main, *The Upper House in Revolutionary America, 1763-1788* (Madison: University of Wisconsin Press, 1967); D. J. Rothman, *Politics and Power: The United States Senate, 1869-1901* (Cambridge, Mass.: Harvard University Press, 1966); S. H. Aronson, *Status and Kinship in the Higher Civil Service* (Cambridge, Mass.: Harvard University Press, 1964);

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B. Bailyn, *New England Merchants in the Seventeenth Century* (Cambridge, Mass.: Harvard University Press, 1955); C. W. Mills, *The Power Elite* (New York: Oxford University Press, 1956); P. M. G. Harris, "The Social Origins of American Leaders: The Demographic Foundations," *Perspectives in American History*, 3 (1969), pp. 159-346. For bibliographies of the mass school, see n. 12 above.

55. See M. Clubb, "The Inter-University Consortium for Political Research: Progress and Prospects," *Historical Methods Newsletter*, 2 (1969).

56. The first abortive attempt to launch this project was in 1929, when an official committee was set up by the House of Commons to investigate "the materials available for a record of the personnel and politics of past members of the House of Commons from 1264 to 1832, and the cost of desirability of their publication." The committee reported favorably and in the 1930's Colonel Wedgwood produced two volumes on M.P.'s between 1439 and 1509. Unfortunately he failed to publish the third volume of synthesis and in any case his methods were so criticized that further work along these lines was abandoned. J. C. Wedgwood, *History of Parliament, Biographies of Members of the Commons' House, 1439-1509* (London: His Majesty's Stationery Office, 1936-1938). Review by M. McKisack in *English Historical Review*, 53 (1938), 503-506.

57. E. Le Roy Ladurie, N. Bernageau, and Y. Pasquet, "Le conscrit et l'ordinateur: perspectives de recherches," *Studi Storici*, 10 (1969). Recent French studies of elites include: F. Bluche, *Les magistrats du Parlement de Paris au XVIIIe siècle* (Paris: Les Belles Lettres, 1960); A. Corvisier, *L'armée française de la fin du XVIIe siècle au ministère de Choiseul* (Paris: Presses universitaires de France, 1964); L. Girard, A. Prost, R. Gossez, *Les Conseillers Généraux en 1870* (Paris: Presses universitaires de France, 1967).

58. See J. E. Neale, "The Biographical Approach to History," in his *Essays in Elizabethan History* (New York: St. Martin's Press, 1958), pp. 229-234.

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FELIX GILBERT

Intellectual History: Its Aims and Methods

THIRTY-ONE years ago Perry Miller published his *New England Mind*. Hardly noticed at the time of its publication, and perhaps even viewed with some distrust as an audacious synthesis, the book was gradually recognized for its originality and has now become a classic. Miller himself was aware of the nonconformist nature of his undertaking and he justified his project and its method in a long foreword; he said that the book was intended "as the first volume in a projected series upon the intellectual history of New England." Was Miller aware that intellectual history was a novel concept, and by using this term did he want to indicate that his work belonged to a new genre of history?

Miller's reasons for using this term can no longer be established. The fact is that in 1939 the term "intellectual history" had not yet become a household word; it has crept gradually into the scholarly vocabulary. In 1904 James Harvey Robinson, one of the founding fathers of what then was called the "new history," offered a course entitled The Intellectual History of Western Europe, and Robinson referred to intellectual history at various occasions.[1] During the interwar years the term came more and more into use, but Perry Miller's *New England Mind*—and this statement is made with all the reservations with which statements about first occurrences must be made—seems to have been the first serious scholarly work which claimed to be an "intellectual history."

An examination of the history of this term reveals some further surprising facts. *Histoire intellectuelle* is not used by French scholars; nor does the *Oxford English Dictionary* recognize the existence of the term "intellectual history." The practice of the *New Cambridge Modern History* attests to the exactitude of the *Oxford English Dictionary;* none of the chapters of the *Cambridge Modern His-*

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tory is entitled "intellectual history,"[2] although many of them deal with subjects for which this term would have been appropriate. In Italy in 1953 the term *storia intellettuale* was still such an unusual combination that it was placed in quotation marks to indicate that these two words might serve best to reproduce the meaning of the German *Geistesgeschichte*.[3] The German knows only *Geistesgeschichte*[4] or *Ideengeschichte*, not *intellektuelle Geschichte*. The failure of historians of other countries to have an equivalent for what in the United States is called "intellectual history" does not mean that the subject matters which come under this heading are disregarded in other countries. In a discussion of the problems of intellectual history the first question, therefore, must be whether Americans have only given a convenient, all-encompassing name to a number of different but related subject matters that in other countries go under various headings, or whether the new name indicates the emergence of a new genre of history.

Unquestionably the subject matters which fall into the sphere of intellectual history have always been concerns of the historian. He has always studied the progress of scholarship—either by examining the history of universities[5] or by tracing the evolution of different scholarly fields—philosophy, law, economics; he has always treated with particular attention and care the development of his own field—the history of history. Works on these subjects have taken different forms; they have focused on individual scholars; they have analyzed change and progress in method; or they have been concerned with the ideas determining these developments—with the history of political thought, of economic thought, of religious thought, and so on. Such studies can also proceed horizontally, encompassing several or all fields of thought of an epoch; their results might be presented in a national framework, as Leslie Stephen did with his *History of English Thought in the Eighteenth Century*, or such studies might attempt to delineate a movement which extended over the entire European intellectual scene; the Enlightenment has been a preferred subject matter of this latter type.[6] The great intellectual movements which formed the dominant outlook of an entire period tie different ideas together in a unique combination, but the various strains of this combination can also be isolated and studied separately. We have had histories of the idea of rationalism, of the idea of progress or of secularism. Usually, investigation of the history of an idea has included the further question of its influence; the problem of the relation between thought

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and action has been behind all such studies even when such a relation was by no means obvious. For instance, since Max Weber and Ernst Troeltsch raised the issue, the relation of religious thought to social life and economic activities has been a source of scholarly disputes. Because of the central role given to political history the discussion of ideas on politics has been a permanent theme in historical work. This concern has led to somewhat massive attempts to demonstrate that in the course of history man has always been dominated by one idea: for instance, that of extending the realm of freedom; or, on the other extreme, to very detailed and sophisticated investigations like those of Friedrich Meinecke in *Cosmopolitanism and the National State,* which carefully analyzes the intellectual influences that formed the mind of the active statesmen in one brief period of history.

It is evident that, insofar as the subject matter is concerned, intellectual history is nothing new; if the emergence of this term in our language represents a new departure it must be the manner in which it is treated that is new. And—as I shall try to show—this indeed is the case.[7]

The problem with which Perry Miller deals in his *New England Mind* is similar to that which forms the subject of Troeltsch's *Social Teachings of the Christian Churches:* The influence of religious beliefs and attitudes on social life and social action. In both cases the sources are primarily theological writings. Troeltsch examined the statements in these writings that had a direct bearing on social life and conduct. Miller reconstructed the entire system of thought of Puritan writers and teachers. He analyzed the logic with which they argued, their image of the cosmos, and their views on the nature of man; God's commands for the ordering of human society were presented as integral parts of the Puritan world view. Whereas Troeltsch assumed that intellectual developments in one field can be treated independently from those in other areas, Perry Miller demonstrated that a change in one area of thought involves a realignment of thought and action in all other spheres. The assumption on which Miller's "intellectual history" is based and which distinguishes his treatment from previous ones is that of the interconnected character of man's concept of life.

A broadening of the scope of investigation can be noticed also in another field of intellectual history—that of the development of the various branches of scholarship. Studies in the history of history usually focused on great historians. They analyzed the advance-

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ment in method from one to the other and explained the progress in historiography by stressing the influence of the philosophical ideas of an age on historical writing. A prime example of this approach has been the historiography of the eighteenth century. The philosophes, as critics of despotism and power politics, stressed the importance of developments in the fields of thought—in art, philosophy, and science; according to them history ought to be history of civilization. Belief in man's natural reason provided criteria with which historians could gauge the defects of political institutions and could measure progress. The philosophical rationalism of the Enlightenment implied skepticism about Christian legends and classical myths and induced a new critical approach to historical sources. The views of the philosophes about the task of the historian were neatly summarized by Voltaire in the statement: "Il faut écrire l'histoire en philosophe." Indeed, scholars who studied eighteenth-century historiography—for instance, Wilhelm Dilthey or John B. Black[8]—chiefly analyzed the influence of the philosophical ideas of the Enlightenment on the historical writings of the epoch. Briefly, they tried to show that the eighteenth-century historians had done what Voltaire had asked them to do. Indeed, this approach reveals the basic intentions and the methodical innovations of historical writers like Voltaire, Gibbon, Hume. But it has also become evident that by focusing exclusively on these great literary figures the picture of the development of historiography in the eighteenth century has been distorted or at least has remained incomplete.

A quite different side is revealed in Lionel Gossman's book, *Medieval Ideologies of the Enlightenment* (1968). This work concentrates on what might be called the internal history of historiography: the relation of a historical work not to philosophy or politics but to other historical writings of the time. Gossman examined the great mass of material on historical subjects published by academies, preserved in manuscripts or contained in letters among intellectuals. The book shows that most of the investigations of historical subjects were undertaken in answer to previous ones with which they were closely linked; out of these debates methodical progress and innovations or, as in the case of the subject of Gossman's book, a new view about the significance of the Middle Ages emerged. Gossman also shows the extent to which academies and salons—that is, the places in which scholars exchanged ideas and presented the results of their inquiries—patterned the content and the form of their work. Gossman's book suggests that in the developments of

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scholarship and intellectual life the means of intellectual communication are of crucial importance.[9]

A tendency to stress the links between the mind of the individual and the thinking of the group from which he comes can be noticed also in works dealing with a subject that has always been a favorite theme of the historian: the influence of ideas on politics. When Meinecke composed his *Cosmopolitanism and the National State* (1906) he seemed to strike out on a new line. He discussed the thought of poets, writers, and philosophers for their contribution to the development of nationalism and he then tried to show how these ideas were reflected in the minds of the political leaders who achieved German unification. But forty-five years later, in Federico Chabod's *Storia della politica estera Italiana dal 1870 al 1896,* the canvas was again enlarged. Chabod, on the basis of newspapers, speeches, and pamphlets, analyzed the impact which recent events—the reorganization of the European state system in 1871—had upon the various groups of Italian society. The result was an all-embracing but extremely differentiated picture of the political concerns in all strata of Italian society around 1870. Then Chabod proceeded carefully to investigate the development of each of the leading Italian statesmen and to establish their position in the spectrum of Italian public opinion—where they might have found political associates and where possible political adversaries; this analysis revealed the road which they might take but also the path which was closed to them. The result is a brilliant description of the mind of a ruling group clearly outlined in its particularity but placed in the framework of thought of the entire society.

Unquestionably, recent scholarly work in the area of intellectual history has stressed aspects which previously received little or no attention: the interconnected structure of thought; the internal self-propelling factors in the progress of scholarship as well as the importance of means of intellectual communication for the nature of an intellectual product and the need to extend analysis beyond prominent individuals to all strata of society. Nevertheless, this does not answer the question whether, and if so in what manner, these developments are connected with each other and whether they indicate a new stage in historiography in which intellectual history is recognized as a particular field of research with its own method, a branch of history equal in importance to the traditional genres of political, diplomatic, and economic history. The question to what extent a change has taken place requires some understanding of the

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manner in which topics of intellectual history have been treated in the past. This is a historiographical question which leads us far back to the sixteenth century, to the beginnings of modern historiography.[10]

The outstanding historical works of the sixteenth and seventeenth century are lengthy detailed descriptions of political, diplomatic, and military events composed by statesmen or participants like Francesco Guicciardini, the Earl of Clarendon, or Samuel Pufendorf. In the nineteenth century, when attention of the historian was centered on these subjects, the only historical works of value produced in these earlier centuries were believed to be the writings of these statesmen-historians. It is true that when these histories were published they aroused the interest of rulers and of members of the ruling group. But students at the universities were taught a very different, much less colorful brand of history. The subject of history at the universities remained universal history although the Reformation had forced some alterations in the medieval outline of world history. Philipp Melanchthon had set up a new scheme of world history compatible with the new ideas. He divided history into two areas: the *historia ecclesiastica* was to focus on the history of the Church as signifying the continued existence of a spiritual world; the *historia ethnica* was to describe worldly events, although the existence of a chain of universal empires within which profane history evolved served as a sign that God kept his protecting hand over all humanity and wanted to save all of it. Already the generation following Melanchthon found this two-fold division incomplete and unsatisfactory. In 1583 one of Melanchthon's pupils demanded that the categories of ecclesiastical and political history should be complemented by addition of a further category, that of scholarly history: "Scholasticam historiam dicere possumus, quae versatur circa litteras, artes, diciplinas earumque originem, incrementum, propagationem ac perfectionem demonstrat." The impetus which artistic and scholarly endeavors had received in the fifteenth and sixteenth centuries could not be disregarded. Thus, Francis Bacon, one of the great synthesizers of the humanist and scholarly thought of the previous centuries, took up the demand for an enlargement of history to include developments in intellectual life. For Bacon history which omitted the history of literature and arts "is not dissimilar to a statue of Polyphemus after his eye had been removed."[11] But Bacon rejected separate histories of the lives of scholars or of the development of individual sciences; the *historia*

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artium et literarum, as he called it, ought to be treated as an interconnected whole. He was aware that he asked for a kind of history that did not yet exist and indeed his suggestion found no followers. Perhaps Vico and Voltaire in the eighteenth century based their description of the various epochs of the past on a conviction of the inner unity of all intellectual phenomena. But in general scholars who worked in the field of *historia scholastica* wrote biographies of individual figures of intellectual life or histories of special scholarly fields. Although the importance of the subject was recognized, the historical literature which dealt with the phenomena of intellectual life stood outside the mainstream of historical scholarship.

When finally a closer integration of intellectual developments with the general course of history was attempted, the impulse came from another direction. That part of universal history that was treated in the *historia ecclesiastica* was inevitably drawn into the dogmatic disputes among the Christian churches and sects. Arguments from history were used in justification of the particular doctrines of each of the Christian denominations; ecclesiastical history became imprisoned in the faculties of theology. Universal history now became identical with *historia politica* but the latter had to change its character insofar as it had to take over the most important task which the *historia ecclesiastica* had carried out: it had to endow the historical process with meaning. The developments of history were placed under the rule of ideas; if history had been separated from theology it became allied with philosophy. Philosophers undertook the task of elucidating the nature of the ideas that controlled the historical process. They demonstrated the logic by which these ideas were linked to each other; because ideas dominated the course of history each period of history was tied to the next in logical succession. The names of Condorcet, Hegel, or Comte show the influence of these notions on the historical thinking of the nineteenth century. Even Ranke, a more empirical mind, was imbued with this way of thinking. Although he might have felt uncertain about the human capacity to discern and define the logic of the historical process, he was convinced that each period of history was reflection of an individual idea and that this idea molded its various aspects. Politics, religion, literature, and philosophy all were expressions of the same *Zeitgeist*. The age of "Realpolitik" was the time of realism in literature; in the period of Baroque art the conduct of politics had Baroque features. In most historical works, however, the attempts to establish connections among political, intel-

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lectual, and social life remained superficial. It is true that in Jakob Burckhardt's *Civilization of the Renaissance in Italy* the analysis of literature, poetry, and politics is linked together to an unforgettable picture of an entire period. But this is not only a unique work; Burckhardt was aware that his approach, with its emphasis on cultural history, was in opposition to that of the great majority of historians of his time. They shunned incisive analyses of literary, artistic, or philosophic thought, and developments in scholarship and science were hardly touched. They picked out a figure or a quotation which they considered to be characteristic of the pervasive mode of thought so that some of the discussions of the intellectual aspects of a period were highly subjective; a striking example is T. B. Macaulay's moralizing judgment about the authors of the Restoration comedies: "Whatever our dramatists touched, they tainted." Although intellectual developments were regarded to form a part of history, references to them remained spotty and primarily illustrative.

In the nineteenth century historical scholarship underwent not only the influence of philosophical idealism but it was also patterned by the forces struggling against this notion. In the overthrow of an idealistic world outlook the thought of Karl Marx has been the most powerful force. Whatever view one might have about the relation of the material to the ideal world and the degree of their dependence on each other, it is no longer possible to see ideas as determining events or floating freely above them. After Marx the existence of a close relation between ideas and interest can no longer be doubted and only careful analysis can determine the function of ideas in social life—whether they serve progress or rigidify the forces which inhibit development. Although Marx's criticism of the idealistic view of history has been his most significant contribution to the development of historiography, among his other contributions there are two which need to be mentioned in this context. One is the emphasis which he placed on the notion of structure.[12] Marx suggested that a change in one element of a social formation presupposes changes in every other element. This view implied the existence of a basic unity in man's attitude toward the various aspects of life. Furthermore, Marx's theory of the class struggle meant that a fundamental gap existed between the intellectual concerns and the outlook of the ruling class and the outlook of the lower classes, the ruled. Because society consisted of various groups with different interests the existence of a common dominating idea in each histori-

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cal period could not be maintained, but the outlook of each period had to be conceived as a differentiated structure. Once this thesis had been advanced it became evident that the attention of historians had been exclusively focused on the ruling class and the actions of its members.

> Denn die einen sind im Dunkel
> Und die anderen sind im Licht
> Und man siehet die im Lichte
> Die im Dunkeln sieht man nicht.[13]

Exclusive concern with the upper classes particularly has shaped the historian's interest in intellectual and cultural phenomena. Scholars have concentrated on the various manifestations of high culture literature, painting, music—but those of popular culture or of the culture of the lower classes—popular religion, music, amusements—have hardly been found worth considering. This was more than an omission, it was a distortion because it disregarded the importance of the interaction which continuously takes place among the various levels of intellectual activities. The simultaneous existence of different levels of intellectual life might lead to modifications of each of them through interpenetration, or it might have the opposite effect of hardening each group in its own position. These questions can be answered only if the past is considered in all its aspects, not only in parts.

Marxism was not the only intellectual movement which undermined the presuppositions of an idealistic view of history. Positivism and the rise of the natural sciences with its emphasis on empiricism worked in the same direction of destroying the assumptions of an ideal reality behind the phenomenological world. By the end of the nineteenth century the notion of history as a process reflecting ideas and controlled by them was abandoned. What happened in historical scholarship was a narrowing of the area of scholarly concerns: research focused on documentary sources and history became exclusively political history.

This is a simplifying description of a somewhat more complicated development. The names of Oswald Spengler and Arnold Toynbee indicate that interpretations of universal history based on Hegelian foundations were not entirely abandoned. However, such wide-ranging projections had their life now outside professional historical scholarship. Interest in the history of ideas also remained alive although they were considered to exist in wide

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distance from the mainstream of history and the investigation of their history became a special field of study.[14] Severed from the entanglement in metaphysical logic, each idea could be viewed as a unit in itself and examined in isolation. An idea might broaden or contract according to the association in which it was placed and the use to which it was put. Such investigations can show how the same idea can have distinct meanings in the minds of different writers; on the other hand insistence on the key importance of the same idea can serve to establish a common trend among intellectuals working in different fields. But the value of such studies, at least for the historian, is limited because they do not include those questions in which he is particularly interested: relation of idea and interest; impact of thought on action; the connection between intellectual attitudes and social levels; and so on.

The fact is that by the end of the nineteenth century the linkage between political and intellectual activities which the belief in the power of ideas over history had established was broken. The professional historian tried to proceed in an almost scientific manner. His traditional inclination toward political history became reinforced by the conviction that the sources for political history were almost the only sources which could be scientifically explored. For political developments, diplomatic actions, constitutional changes were fixed in written documents. These areas were regarded as the historian's legitimate concern.

It is a not uncommon error to assume that criticisms of the narrowness of political and diplomatic history are of recent date. Actually, they go back to the last decade of the nineteenth century;[15] since then a discussion has been underway about the historical significance of the social and cultural factors which, with the decline of historical idealism and the concentration on political history, had been sloughed off the main body of historical work. The genesis of the new history in the United States belongs in this context;[16] in a country in which religious convictions and economic expansion had been powerful historical forces the limited nature of a politically oriented historiography was particularly noticeable. A further reason was the importance that the social sciences were gaining in the United States; by stressing the role of social forces in history, history would be brought into the orbit of the social sciences. In any case it was of great significance for the rise of intellectual history in the twentieth century that, because of the exclusiveness with which the historians practiced political history, the orphaned

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fields of social history and intellectual history were coupled together. This relationship greatly influenced the evolution of intellectual history: it served to sharpen questions and to refine methods by directing attention to the methods and advances in the social sciences; it represented a spur to a clear definition of the task of the intellectual historian. The differences between older and more recent studies in the field of intellectual history to which the earlier part of this essay directed attention can now be seen as a result of the increased awareness with which research in this field had to be pursued. Changes took place in all areas of intellectual history—in the investigation of the mind of an individual, of the outlook of a group, or of the origin and the impact of an intellectual product.

The biography of an intellectual figure has always been recognized as a legitimate subject; the changes in this field, therefore, are particularly startling. In earlier times a man's ideas and thoughts were presented as parts of a system which he had formed in his early years and to which he held throughout his life. Early statements could be explained therefore by statements made in later life and vice versa. Or his system could be divided into its component parts and one chapter could summarize his views on moral philosophy, another on economics, and a third one on politics.[17] The scholar who broke with this approach was Dilthey, first in his *Leben Schleiermachers* and then in the *Jugendgeschichte Hegels*. Dilthey regarded mental attitudes as stimulated by external impressions; the mind of a person was nothing constant; it was in flux, changing and developing. Therefore in dealing with the life and the works of an intellectual figure a historian must follow the intellectual evolution of his hero from step to step. The written material from each period of life must be examined separately. No idea ought to be explained by reference to statements made at later times. One can understand the significance of an intellectual choice only if one is aware of the possibilities which existed. All this seems obvious, but if earlier times were not aware of what has now caused an excited debate, the distinction between the humanist Marx of early years and the materialist Marx of later years, the reason is that they did not know the genetic method.

The application of new methods is closely tied up with the opening up of new sources. In the writing of the biography of an intellectual figure, material which had seemed of secondary significance—drafts, fragments, lecture notes, private correspondence—

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have gained importance because they might indicate the gradual maturation of ideas.

Any intellectual work is the product of its author's mind, but it is also shaped by its function. It is part of an intellectual debate. It answers questions which have previously been raised. It is presented in the form in which such problems have previously been treated. Consciously or unconsciously the writer has in his mind a model according to which he fashions his work. This has frequently been overlooked in the past. If people are astonished by the moralistic tone in general histories of the nineteenth century the reason is that they are no longer aware that history was taught at the universities as part of moral philosophy; such an original work as Burckhardt's *Weltgeschichtliche Betrachtungen* stills shows the traces of an adjustment to the moral purposes of history teaching. Briefly, it has become clear that no intellectual work can be really evaluated without knowledge of the model according to which it was patterned. Of course, great works and great achievements are those which break through the traditional mold and create a new trend. But an exact appreciation of what is new and has been achieved requires knowledge of the shell that was broken.

In many respects the methods available for the analysis of the evaluation of the mind of an individual and for the establishment of the distinctive intellectual features of a group are the same. But there are also methods particularly appropriate to the one or the other. If the genetic method is especially suited for a biographical study, a chief requirement in the discussion of intellectual developments and trends is the analysis of key concepts. Writings may differ in purpose and content; in order to demonstrate whether their authors have a common intellectual outlook, how one group differs from another, or what constitutes the dividing line between intellectual movements, it is necessary to find out to what extent the key concepts of their conceptual framework were identical. Bernard Bailyn, in his analysis of American pamphlets before the War of Independence, emphasized their common notions about the nature of power and the nature of liberty and concludes that these pamphlets showed an "integrated group of attitudes and ideas which made concerted action against the English rule possible."[18] There are many issues, of course, from which the common outlook of a group or intellectual divisions in a society can be inferred; the positive or negative evaluation of periods of the past—of the classical world, of the Middle Ages—has been frequently and efficiently

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used as such a touchstone. Probably it would not be right to claim more than that in cases where previous generations of scholars have trusted their intuition the present tendency is to be exact and detailed. The present manner of quoting might serve as an example. If a historian deals with the works of an individual, a striking and concise passage is in place. But when the thought of a group is discussed, the representative statement that can be backed up by a large number of similar statements is necessary and if footnotes with references to this mass of similar passages become too bulky the scholar will have to follow Perry Miller who placed into the library a complete set of notes "in which references are given . . . to other instances of like utterances."[19]

We have said that the interest of the intellectual historian has extended beyond the ruling group to all classes of society; this widening of his field of research requires new methodical procedures. In popular literature the contents, the ideas, are of secondary interest. They are simple and repetitive. The historian wants to know about the influence of this kind of literature and this question requires investigation of the size and number of editions, their regional distribution, and so on. This is a field for statistical investigations and has become more manageable through recent technical inventions. But we are only at the beginning of establishing the possibilities and limitations of statistical methods in this area.

In the case of popular literature it is clear that its contents is determined by the audience to which it is addressed. This can be said, however, about almost any intellectual product. Every intellectual activity wants to make some kind of appeal and is shaped by this need. In the past historians have rather neglected these questions and they have learned about their importance chiefly from studies in the field of sociology and political science. Clearly the decisive change in external conditions of intellectual life was the invention of printing. Whether a manuscript circulated among a small number of people with education and interests similar to those of the writer, or whether a manuscript might be read by a great number of people unknown to the author creates necessarily a great difference in the attitude of an author. The decline of Latin and the rise of the vernacular, the abandonment of a chancellery language in public documents in favor of the spoken language, these consequences of the invention of printing stimulated new developments in literary life and political thought. Moreover, printing was a business and writing became a profession. The invention of print-

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ing also placed the question of social control of intellectual and literary activities on a new level. It was the state and the government rather than the Church which by means of licenses and censorship could direct or influence the direction of intellectual work. The institutional aspects of these developments are well known but it is still somewhat *terra incognita* to what extent these developments shaped the course and the tempo of intellectual activities.

Methodical procedures are a scholastic subject; they have entered this discussion because, and insofar as, they reveal a characteristic trend of recent work in the field of intellectual history: search for precision. This tendency is a response to the objection which the intellectual historian frequently encounters: that he operates with vague terms like "influence" or "dominating trend," that his product lacks in precision and is "unscientific." It may have to be admitted that in work of an interpretative character intuition will always have a part, but although there may have been reasons for complaints about the lack of exactitude in intellectual history in the past, this negative attitude is hardly justifiable with regard to recent work done in this field.

Unquestionably the recognition of the need for carefulness and precision in intellectual history owes something to the fact that the intellectual historian wants to be able to compete with the social historian. The development of the relation between intellectual history and social history is interesting and enlightening. In a very general way they are complementary. In their modern form they emerged at the same time and as allies, both struggling against the domination of history by politics. This close relationship is reflected in the fact that at American colleges and universities courses bearing the double title Social and Intellectual History have been frequent.[20] But when their fight for recognition as essential branches of history had been won alliance changed into antagonism. One reason was the opposite nature of their subject matter. The social historian focuses on movements whose driving force frequently remains anonymous: demographic changes, economic growth, technological advances. The work of the intellectual historian remains tied to the human mind and its products. The difference in subject matter has created also a more profound contrast. The social historian tends to imply that a full analysis of the facts with which he is concerned might "explain" history and even indicate future trends. An extreme expression of this attitude is the view that the

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course of history is determined by laws which can be discovered. In a more cautious form the same idea can be found in the demand that the historian should concentrate on basic structures or forces of long duration like climate and geography; in relation to them a history of events deals with factors that are ephemeral.[21] The implication is that only social history comes to grips with the basic factors of history; only social history is "true history." This claim diminishes the value of all other branches of history and necessarily evokes the opposition of their practitioners.

The contentions of social history are of significance in our context for a special reason. They throw a sharp light on the general position of intellectual history and of the intellectual historian. Intellectual history cannot claim to be the true or only history; modern intellectual history arose after belief in the control of events by ideas had collapsed. It exists only in connection with, and in relation to, the surrounding political, economic, and social forces. The investigation of subjects of intellectual history leads beyond the purely intellectual world and intellectual history per se does not exist. But if this is so, has the intellectual historian a special task, and what is it?[22] The debate on this question has brought forth various suggestions and the answer most frequently given is that intellectual history is concerned with reconstructing the *Zeitgeist* of an epoch. This solution of the problem is questionable because every society consists of various social strata and each has its own intellectual outlook; if we go beyond reconstructing the outlook of the ruling group the picture becomes diffuse.[23] Moreover, there is still a Hegelian flavor in the assumption that each period has its own *Zeitgeist*. It might be more modest to say that the intellectual historian reconstitutes the mind of an individual or of groups at the times when a particular event happened or an advance was achieved. To call this formulation "modest" is probably an understatement because this task, if fully done, will lead very far afield. Moreover, it is a task of great import. Whatever one thinks of the forces that underlie the historical process, they are filtered through the human mind and this determines the tempo and the manner in which they work.[24] It is human consciousness which connects the long-range factors and forces and the individual event and it is at this crucial point of the historical process that the intellectual historian does his work.

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1. On James Harvey Robinson see Luther V. Hendricks, *James Harvey Robinson, Teacher of History* (New York: King's Crown Press, 1946), esp. p. 16. In James Harvey Robinson, *The New History* (New York: Macmillan, 1912), we find on pp. 101-131 an essay entitled "Some Reflections on Intellectual History," and the *Festschrift* devoted to him in 1929 was called *Essays in Intellectual History,* but for Robinson "intellectual history" seems to have been chiefly identical with a history of the progress of scholarship. For the questions connected with the development of this "branch" of history, see John Higham, "The Rise of American Intellectual History," *American Historical Review,* 56 (1951), 453-471, esp. pp. 458-459. Higham calls Carl Becker's *Declaration of Independence* "the first important contribution" of the new school of intellectual history, but it should be noted that Becker himself gave his book the subtitle *A Study in the History of Political Ideas.*

2. The term "intellectual" appears only in one chapter heading of the *New Cambridge Modern History,* in the combination "intellectual tendencies"; see vol. II, chap. XII.

3. See Delio Cantimori, *Studi di Storia* (Torino: Einaudi, 1959), p. 495, in an article which was first published in 1953. An earlier use of the word (see *ibid.*, p. 227) has not the meaning of "intellectual history."

4. *Geistesgeschichte* is used in different ways; for a recent application of this term, see note 23 below.

5. In a broad sense, the history of education falls into the category of intellectual history, but it has its special features which make a more detailed separate treatment desirable, see the essay by John Talbott in this volume; undoubtedly there are many parallels between the problems of modern intellectual history and modern history of education. On the other hand, the history of science seems a field somewhat apart; for a discussion of this problem, see the essay by Thomas Kuhn in the spring 1971 issue of *Dædalus.*

6. The two volumes of Peter Gay, *The Enlightenment: An Interpretation* (New York: Knopf, 1966-1969) are the most recent examples.

7. I would like to emphasize that the following confrontation of older with more recent works of historical scholarship is not meant to involve judgments of values; there are many factors—above and beyond methodical procedure—which determine the importance of a scholarly work.

8. Wilhelm Dilthey in "Das Achtzehnte Jahrhundert und die Geschichtliche Welt" (now in his *Gesammelte Schriften* [Leipzig: Teubner, 1921-1936], vol. III), and John B. Black in *The Art of History* (New York: Crofts, 1926).

9. For demonstration of this point, I could have also chosen the book by Andreas Kraus, *Vernunft und Geschichte, die Bedeutung der Deutschen Akademien für die Entwicklung der Geschichtswissenschaft im späten 18. Jahrhundert* (Freiburg: Herder, 1963); Kraus places particular emphasis

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on the means of scholarly communication, but he follows very much the same lines as Gossmann, with equally startling results.

10. For the following, see Adalbert Klempt, *Die Saekularisierung der universal-historischen Auffassung* (Göttingen: Musterschmidt Verlag, 1960). It is very characteristic that in earlier research the entire issue of the *historia scholastica* has been overlooked; it deserves more study.

11. Bacon, *De Augmentis scientiarum*, bk. II, chap. 4.

12. See R. Bastide, *Sens et usages du terme Structure* ('S-Gravenhage: Mouton, 1962), esp. pp. 100-106.

13. Bertolt Brecht in the film version of the *Dreigroschenoper*, badly translated by Isherwood as:

> Therefore some are in darkness
> Some are in the light, and these
> You may see, but all those others
> In the darkness no one sees.

14. The program of this "school" has been outlined by Arthur O. Lovejoy, in an essay entitled "The Historiography of Ideas," now reprinted in his *Essays in the History of Ideas* (Baltimore: Johns Hopkins University Press, 1948); Lovejoy's program was broader than his interests which were primarily aesthetic and philosophical.

15. In Germany, a revolt against the concentration on political history broke out in the nineties, and many of the concepts which are now in the center of theoretical discussion—for instance, type, structure, statistical method, situation—made their appearance in this debate; see Gerhard Oestreich, "Die Fachhistorie und die Anfänge der sozialgeschichtlichen Forschung in Deutschland," *Historische Zeitschrift*, 208 (1969), 320-363. The struggle in Germany ended with a triumph of the political historians, at least in the sense that they were able to keep their opponents out of all influential positions. In France, Henri Berr raised the demand for a *synthèse historique* in 1900, and the line runs from him to Marc Bloch and Lucien Febvre and the foundation of the *Annales*. Similar developments can be observed in Italy; only English history seems to have missed out on this controversy. It is a chapter of historiography which has never been treated in its appropriate supranational context.

16. Higham's section in John Higham, Leonard Krieger, Felix Gilbert, *History* (Englewood Cliffs, N. J.: Prentice-Hall, 1965), describes in detail these developments in the United States.

17. See, for instance, Leslie Stephen's treatment of Locke in his *History of English Thought in the Eighteenth Century* (New York: G. P. Putnam's Sons, 1876).

18. See Bernard Bailyn, ed., *Pamphlets of the American Revolution*, I (Cambridge, Mass.: Harvard University Press, 1965), p. 60.

19. Perry Miller, *The New England Mind: The Seventeenth Century* (New York: Macmillan, 1939), p. ix.

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20. See H. L. Swint, "Trends in the Teaching of Social and Intellectual History," *The Social Studies,* 46 (1955), 243-251.

21. On the concept of the *longue durée,* see Fernand Braudel in *Annales: économies, sociétés, civilisations* (1958), now reprinted in Fernand Braudel, *Ecrits sur l' histoire* (Paris: Flammarion, 1969), pp. 41ff. It seems to me that Braudel's emphasis on the importance of factors of *longue durée* has made the gap between structure and event almost unbridgeable. As important and rich as Braudel's *La Méditerranée et le monde méditerranéen à l'époque de Philippe II* is, I agree with Ernesto Sestan, "Storia degli Avvenimenti e Storia delle Strutture," report on the *XIII Congresso Internazionale di Science Storiche* (Moscow, 1970), pp. 24-25, that Braudel never fully succeeds in showing the relevance of the long-range developments for the events of the period of Philipe II. The question, as I shall discuss below, has bearing on the significance of intellectual history.

22. See Franklin L. Baumer, "Intellectual History and Its Problems," *Journal of Modern History,* 21 (1949), 191-203. But see also the review essay of Arthur M. Schlesinger in *History and Theory,* 7 (1968), 217-224, and the article by John Higham, "Intellectual History and Its Neighbors," *Journal of the History of Ideas,* 15 (1954), 339-347, for general considerations of the problem.

23. Hans Joachim Schoeps considers *Zeitgeistgeschichte* as the primary task of *Geistesgeschichte,* but the two volumes in which this notion is supposed to be carried out—*Das Wilhelminische Zeitalter* and *Zeitgeist der Weimarer Republik* (Stuttgart: Klett, 1967-1968)—present essays on various aspects of life, but no synthesis.

24. Marx's statement in his preface to the *Critique of Political Economy* deserves to be quoted: "In considering such transformations a distinction should always be made between the material transformation of the economic conditions of production which can be determined with the precision of natural science, and the legal, political, religious, aesthetic or philosophic forms in which men become conscious of this conflict and fight it out."

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BENJAMIN I. SCHWARTZ

A Brief Defense of Political and Intellectual History . . . with Particular Reference to Non-Western Cultures

I SHALL begin with a confession. In my efforts to study the history of one non-Western civilization—China—I find that I have tended to focus on two areas of history—political and intellectual (as I understand these terms)—which do not currently enjoy high esteem in many sectors of the historical profession. Owing to certain considerations which are not entirely methodological, my interest in political history has largely focused on contemporary China, while my concern with intellectual history has involved both traditional and modern China. There has, however, been a considerable interaction between both interests. In reflecting on the common denominators which may underlie these two areas of interest, it occurs to me that both are, to a considerable extent, involved with the realm of man's conscious life—with his conscious relationship to the situation in which he finds himself and to his conscious behavior within that situation. To the extent that politics involves ideology, policy, decision-making, and even power relations, it involves conscious intention and conscious activity. In this view, even power relations as they appear in political life are power relations as perceived by the actors.

In struggling to achieve a conception of political history, I find myself somewhat in sympathy with the current tendency to downgrade emphasis on the history of political institutions and constitutional history. In the case of those structures known as political institutions, there has emerged a realization of how problematic and complex are the relations between formal structure and the human realities which they supposedly condition and constrain. Certainly, any effort to make sense of the political history of mainland China in the last twenty years could hardly depend entirely on the history of institutions, constitutions, and formal organizations, and yet we

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are dealing with a history in which there is considerable truth in the assertion that "politics are in command." If this phrase has any meaning within the context of contemporary China, it must refer not simply or even primarily to institutions but to categories such as policy, decision-making, power relations, and the interplay between ideas and political action.

At the same time the notion that political institutions are in some sense less real or fundamental than all those institutions and structures called social seems to me to be based on questionable assumptions. If it derives from the view that political institutions are often created and shaped by conscious intention and conscious activity while social and economic institutions are the products of deep, unconscious impersonal forces "independent of the wills of men," it seems to me that the contrast is overdrawn on both sides. Many American political scientists of the more scientific persuasion are as ardently bent on proving that the structures of political life and models of political development are as independent of the wills of men as any of the structures described by the economists, sociologists, and anthropologists. If one is prepared to believe Lévi-Strauss's assertion that "myths think themselves out in men,"[1] one can perhaps say that constitutions and political institutions think and act themselves out in men. On the other hand, the notion that conscious purpose and conscious activity are absent in such preeminently "objective" areas as economics, the history of technology, or anthropology simply rests on unthinking dogma. A close look at the history of extended lineage structures in South China would indicate that the maintenance of lineages over time was by no means due simply to the preexistence of unconscious *gemeinschaftliche* kinship structures, but to the highly conscious, organizational efforts of enterprising clan leaders.[2]

Nevertheless, one need not deny that in the case of political institutions, the role of conscious activity is often close to the surface. In China we are able to follow quite closely and with comparative clarity the collective deliberations which led to the formation of the Chinese Communist party and to the creation and destruction of various organizations which have emerged since 1949. This fact neither adds to nor subtracts from the reality of such institutions. Political institutions are neither less real nor less pervasive in their effects than other institutions, but they by no means provide the sole, or even the primary, substance of political history.

If political history involves conscious activity, it is, of course,

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by no means conscious activity in a vacuum, but conscious activity set within the framework of all the problems, pressures, and constraints imposed by the objective situation. In China one can hardly begin to discuss the programs, policies, decisions, and disputes of the political actors without constant reference to such factors as the overwhelmingly agrarian nature of China's economy, the crushing weight of its population, the persistence of cultural patterns inherited from the past, the contingencies of China's relations with the outside world, and so on. The political historian must welcome all the aid which economists, demographers, anthropologists, and others can provide to help him to understand the situation within which political action takes place. It is by no means demonstrated, however, that the responses of the political actors are the only obviously necessary effects of any or all these forces. It is thus quite clear that during the years 1960-1965 sharp differences of perspective developed within the Chinese Communist leadership concerning the policies to be pursued relative to the socioeconomic and cultural situation which had emerged after the Great Leap Forward. One might, of course, explain these divergent perspectives wholly in terms of power struggles within the leadership. Even if one were entirely prepared to settle for this account of the motivation of the various parties involved, the fact remains that power interests became indissolubly linked to different policy perspectives and that these perspectives in turn reflect divergent images, perceptions, and hopes on the part of the political actors involved. To assert that Mao Tse-tung's Cultural Revolution was the only obvious response to the imperatives of the objective situation of the early sixties is simply to indulge in spurious hindsight.

I am not here trying to prove the case for the freedom of political actors in the philosophic sense. If their actions are not clearly determined by the external objective processes, they may still be determined by internal structures and processes. The social backgrounds of the political actors, their personal biographies, their personality structures, and even the role of ideology as myth in Lévi-Strauss's sense may all be brought in to explain their conscious acts and decisions. One may psychoanalyze Mao Tse-tung. The point is that these forms of determinism are mediated through the conscious political acts of the political actors. This leaves quite open the possibility of the relative autonomy of the political sphere. It also leaves open the possibility that political acts and decisions may substantially affect the course of history.

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Here, however, we confront another ground for asserting the superficiality of political history—namely, that political history places undue stress on the acts of tiny groups of people or even on the acts of single individuals. It thus lends aid and comfort to the "great man theory of history" and is elitist in its very essence. It is an offense to both populist sentiment and to science—to populist sentiment in that it denies that the important movements of history are the cumulative effects of the behavior of vast masses of men[3] and to science because the general "laws of history" must manifest themselves through vast masses of instances.

It is, of course, entirely true that the data on princes and prime ministers is infinitely more accessible than data on the political history of provinces and villages. This does not mean, however, that there is no political history of villages. Patrice Higonnet has written a dissertation on the social and political history of the village of Pont de Montvert in southern France which indicates a complicated political life within the village as well as a complicated dialectic of relationships between the political and intellectual history of the village and of the nation as a whole. Frederick Wakeman has written a study of the political life of eastern Kwangtung province in the wake of the Opium War which lifts the veil on the enormous complexity of the local political history of one small area of China.[4] Here we find an amazingly intricate web of political relations between local officialdom, clan politics, secret societies, local gentry, and interethnic rivalries. Other young scholars in the field of modern Chinese history have turned their attention to the political history of provinces and cities during the period before the 1911 Revolution and emerged with extraordinarily circumstantial and lively accounts of local political history. All of these accounts combine political history with social history. The authors must make an effort to reconstruct the physiognomy of local social life but it is the dynamic and conscious life of local political history which reveals the concrete human reality of these social relations. There is no reason to think of political history as simply the history of the policies of kings or courts. It may just as well involve the frustration and deflection of policies of leaders in the capital by local power constellations and the local politics of villages. The policies associated with the Cultural Revolution in China were undoubtedly initiated by Mao Tse-tung and a small group of his supporters, but the turbulent evolution of China during the last three years has been as much due to the unanticipated

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deflection of these policies by various political forces as to the will of the leader himself.

To be sure, even though the web of political history is spread wide, it may be asserted that for the larger part of human history the masses of mankind have not participated in political life and that the political actors, whether they are monarchs or local clan leaders, are almost by definition part of the ruling class. Much, of course, depends on what one means by participation. If one concedes that political acts and political events may have profound and even devastating effects on the lives of men, then the masses certainly have participated in political history at least as victims. In fact, they have participated in many other ways. The stereotype of the Chinese peasant village that lives a timeless life while the waves of political history flow over it may have a certain partial truth for certain times and certain remote places, but it is essentially not true. All of us know—at least in the abstract—about the vast peasant rebellions which periodically swept China affecting and involving vast populations. We also know about conscription of vast hordes of men for military activities and corvée labor far from home. We know of banditry and secret societies and of the voluntary and involuntary participation of even humble peasants in the bitter feuds of local political life. We also know that peasants did not necessarily remain immured in their villages but were frequent visitors to market towns where they undoubtedly exchanged information concerning local political situations and perhaps even concerning the state of the empire. We know about many of these things in the abstract but, at least in the case of China, what we need if we are to clothe these abstractions with flesh is much more attention to the much despised *histoire événementielle*, to the extent that the Chinese sources are able to supply us with materials. While it is true that all of these sources may be limited from the point of view of certain kinds of questions which we would like to address to them, the fact is that we have hardly begun to exploit what they do contain.

The fact that political history tends to be closely associated with history as story—with the history of events—is, of course, another ground for asserting its superficiality. It is quite true that much political history has been concerned with the main events and the actions of the most visible actors, but presumably even a comprehensive political history which would provide an account of all political actions and events down through every province, city, and

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village would still be superficial in this view because it would still be a history of surface events rather than of the secular movement of deep, underlying, unconscious structures. I shall not dwell here at length on the procedures by which the variables which enter into these accounts of deep, impersonal forces are assigned an independent life of their own as the prime movers of history. It is thus possible to collect all the statistics available on a certain network of trade over a given period of time (no doubt this is a very valuable enterprise from many points of view), to suppress all considerations of political events, military events, shifts in mentalities, and other variables which may have profoundly affected this statistical series, and to emerge with the view that this pattern of trade is an absolutely independent "infrastructural" variable.

Similarly, one way of dealing with the history of mainland China in the last twenty years is to assert that the only significant fact is the fact that China is undergoing a "process of modernization," operating, as it were, behind the backs of all the participants, and that all the shifts of political history are simply surface eruptions of this process or, if not, essentially insignificant. I shall not dwell here on the obscurities of the concept of modernization. It is based on the assumption that there exists an all-encompassing and apparently stable structure of something called "modern society" which provides the clear and unmoving target of all developing societies (since all development is development toward something). One need not deny that all societies now called modern share certain features in common, the most indisputable being a highly industrialized economy. Yet the notion that the differences between these societies are insignificant or the notion that these societies have achieved a kind of plateau of essential stability which will preclude sweeping unpredictable transformations in the future is part of what might be called the ideology of modernization. The target itself is neither clear nor unchanging. Modern societies of the twenty-first century may exhibit features which run entirely counter to the dominant trends of the mid-twentieth century. Without denying for a moment that the political leaders of China are interested in many of the elements which we tend to subsume under the category of modernization, the question of how China will modernize holds many obscurities and uncertainties for the leadership itself. The question of how to modernize becomes a political question and the political decisions of the leaders may have a profound effect on the nature and tempo of China's modernization.

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The Bolshevik seizure of power in October 1917 was a political act and part of the history of events. Can one contend that this event had no effect on the history of the Soviet Union during the last fifty years? One need, of course, assume that the intentions of the original actors have been realized. Political intentions and political acts like ideas may have unintended consequences. Yet even the unintended consequences are inconceivable without the original action. At this point in time, it seems to me entirely reasonable to say that the political act of 1917 was one factor of enormous importance in shaping the history of the Soviet Union. Whether it will have any significance whatsoever a thousand years from now is a moot point. It is just as unclear, however, whether our present constellations of demographic, economic, and sociological trends will have any significance a thousand years from now. In the time scale of millennial history, single political acts and policies may shrink in significance, but this leads us to the question of whether the type of history which deals in millennia is necessarily more profound than the type of history which deals with the time scale of a generation or two. In the latter scale, political history retains its perennial significance. To men who happen to live in the time scale of one generation, politics are always with us.

There is in fact one way of regarding political history (and other forms of history as well) which runs quite counter to nineteenth- and twentieth-century historicism and reflects certain older ahistorical ways of looking at history. Political history is here regarded not as a single irreversible process but as a reservoir of instances of political experience. It provides the possibility for a kind of diachronic metahistorical and transcultural comparative politics in which it no longer is heretical to compare something in ancient Roman history to something in modern Chinese history provided that this is done with due deference to the insights which arise out of the "modern sense of history." Thus political actions and political events are significant not only for their place in some single world historic drama but also as comparative material for the study of man in politics in various times and places. This is not to deny the emergence of genuine novelty or the possibility of progress. It is simply to insist that together with novelty one also has the constant recurrence and recombination of problems and motifs of perennial politics.

Turning from political to intellectual history, as already stated, a common element linking the two interests seems to me to be the

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concern with the realm of man's conscious life. The English term "intellectual history" is most unfortunate since it seems to imply an exclusive concern with the intellect in the narrow sense of the term and hence also seems to imply an exclusive concern with the history of those called intellectuals. Intellectual history, as we conceive of it, involves the totality of man's conscious life—the life of the intellect, the emotions, the imagination, and every variety of sensibility—and not simply the realm of conceptualization. Furthermore, we are by no means exclusively concerned with the intellectual life of man as a self-subsistent realm—as the so-called "history of ideas"—but with human consciousness as related to the historic situations in which men find themselves. To use Merleau-Ponty's trenchant phrase, "To be conscious is, among other things, to be elsewhere."[5] Ideas themselves may be concerned with matters which are entirely material. This view of intellectual history by no means precludes the history of ideas. It cannot be denied that the intellectually articulate in relating to their life situations tend to relate not only to life in its unreflective immediacy but also to the ideas of others about life. One must always understand their thought in relation to the thought of both their predecessors and contemporaries.

Like political history, intellectual history has been subject to the charge of elitism, although in my own view with even less justification. While political action may be confined to small groups, even though the actions themselves affect the society as a whole, the conscious life of man is omnipresent wherever men are to be found. I am, of course, entirely aware of the academic conventions which assign the study of collective mentalities of the masses to sociology or cultural anthropology or social history. It is indeed possible to subsume even the intellectual history of intellectuals under these categories. Thus Lévi-Strauss is quite prepared to treat some of the ideas of his great contemporary Sartre under the category of anthropology.[6] Yet even those who concede a certain autonomy of intellectual history as the history of the ideas of reflective intellectuals, insist on appropriating the mentalities of the masses for the social sciences. Presumably one basis for this assignation is that any phenomenon which is collective must seek its aetiology in sociology or some social science, although Norman Cohn and other psychohistorians have felt it quite possible to explain collective mentalities in Freudian psychological terms. More important, however, is the assumption that, whatever may be the

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case for intellectuals, the minds of the broad masses may be thought of as passive receptacles which are only able to reflect the mentalities produced in them by vast impersonal societal and cultural forces. Lévi-Strauss assigns a considerable causal weight to collective mental structures but the point is again that these structures, like language, are objective autonomous realities which manifest themselves through the essentially passive receptacle of the individual mind. To be sure, in the case of Lévi-Strauss, it is by no means easy to pigeonhole his thought. In his recoil from Sartre's contrast between the modern dialectical thinker (like Sartre) who is actually able to think and the primitive man whose conscious life is wholly controlled by preestablished structures, he tends to deny the distinction on both sides and points to the broad anthropological evidence concerning "primitive philosophers" who are quite capable of thinking through and justifying in their own terms the traditions of their tribe on a highly conscious level. If this is true of primitive men, it need not be less true of the mentalities of the masses of men in the so-called higher civilizations. The notion that the mentalities of the masses as opposed to the reflections of intellectuals are wholly passive and nonreflective and easily explained in terms of the social models which we impose upon them is not based on any genuine evidence. Whatever the claims of sociology, anthropology, or economic history, there is no reason whatever why the study of the conscious life of the masses should not also be subsumed under the category of intellectual history. It is quite true that intellectual historians of China have so far devoted far too little attention to movements of popular religion, to the "ideologies of rebellion," to the interweavings of mythology and cosmological thinking on the popular level, and a host of other subjects, but they should certainly consider research in these areas as part of their task.

Indeed, one of the barriers to a broadly conceived intellectual history of this type both in China and elsewhere has been the facile acceptance of the distinction between "folk culture" or "little culture" and "high culture." The distinction no doubt has validity as a heuristic device, but the notion that there has anywhere been an iron wall dividing the two has been most misleading, particularly in the case of China where, ever since the eighteenth century, there has been a tendency in the West to contrast the cool rationalistic Confucianism of the literati to the irrational superstitions and primitive folk religion of the masses. In fact, a closer look reveals that the literati most often shared in the superstitions and magical be-

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liefs of the masses and were indeed able to provide a kind of rational basis for these beliefs in their highly developed cosmological theories (as in the case of geomancy). To a greater or lesser degree, they also shared in and were able to provide rationales for their participation in various aspects of so-called popular religion. On the other hand, popular literature, proverbs, and other more immediate sources of evidence would indicate that the ideas of the so-called high culture circulated widely among the masses. The ideologies of rebellion are by no means simple products of the folk mind. They were most often fashioned by literati or semiliterati but were, of course, quite accessible to the masses. As in the case of the rise of Christianity or the Reformation, there have been vast movements of the spirit which have cut across the whole society. To be sure, the modalities of acceptance of these movements by various strata and groups within the society could differ most markedly. The same religious and ethical concepts could be part of Establishment culture and also figure in the ideology of rebellions. To be sure, Buddhist philosophic scholasticism and Neo-Confucian scholasticism, like all scholastic refinements of doctrine everywhere, remained the preserve of the monk and literatus, but this does not mean a priori that there was nothing in common between the religion of the monk, the literatus, and the peasant.

One reason for assigning the collective mentality of the peasant village to the anthropologist is the assumption that peasant villages like primitive tribes have at some point in time reached a stasis of cultural forms which makes both political and intellectual history largely irrelevant. This, again, is an unproved assumption. There is thus considerable evidence that during the T'ang dynasty, when Buddhism was a vital encompassing force in Chinese life, the life of a large part of village China was dominated in all its aspects by Buddhist beliefs, festivals, and the institutions of Monastic Buddhism, while in later centuries, when Buddhism had retreated as a vital force, although one still finds definite Buddhist elements in the diversified folk culture of China, the spirit of popular culture had changed.

However, while intellectual history as here conceived in principle lays as much claim to the mentalities of nonintellectuals as to the ideas of intellectuals, the fact remains that we have most easy access to the ideas of the literate and the articulate. Nor can there be a denial of the fact that for the greater part of human history (including the present) the literate and intellectually articulate

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have belonged to the relatively more privileged sectors of society. One may say that they have more or less belonged to the Establishment (particularly in the case of China's gentry—scholar-officials) or at least been appended to it, as in the case of modern academic intellectuals. Hence, it can always be alleged in a quasi-Marxist spirit that their ideas either consciously or unconsciously reflect the interests of the ruling class. It is also frequently alleged that the intellectuals tend to concern themselves with matters which are of exclusive concern to themselves and not to the bulk of mankind. But intellectuals, unless they are blatant propagandists, have always regarded themselves as being engaged in a truth-seeking or truth-proclaiming enterprise designed to cope with the mysteries of man and the universe. They have thus always implicitly attributed to their own conscious efforts a certain degree of transcendence over the various class interests, psychological motives, and other factors which can be viewed as the hidden springs of their behavior. Indeed, most of them have tended to assume that their ideas could even have effects on the world surrounding them.

This, of course, is no place to attempt to confront the enormous philosophic problems involved in giving an account of man which tries to pay due deference to objective forces, to interest motives, and to psychological motives while taking seriously his claims as a truth-seeking animal. All I would suggest is that we ourselves are as deeply involved in these dilemmas as any of our predecessors and that the devices used to separate our own intellectual enterprises from theirs will simply not bear scrutiny. One such device is to assume that at a given point in the historic process, it suddenly becomes possible for certain types of men to free themselves from the trammels of genetic conditioning and to confront the truth in all its fullness. To Hegel, the self-realization of the World Spirit coincides with his own lifetime. To Marx, the rise of the proletariat as a class which will finally overcome all the false consciousness of the past confirms the truth of his doctrine, while to Karl Mannheim his own life witnesses the final emergence of a free-floating intelligentsia whose views are no longer distorted by partial interests. Many American social scientists are convinced that they achieved the happy state of scientific objectivity in the affluent fifties. The procedures here are almost too transparent to require comment.

Another way to achieve this appearance of transcendence is to lay claim to the precious mantle of science. Lévi-Strauss would have us believe that the purity and rigor of his science somehow lifts

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him above the cultural structure in which he is involved. What is claimed now is that certain theories, ideas, and hypotheses have finally been, or are about to be, verified by the facts, thus placing these theories and hypotheses in an entirely different category from all the ideologies and theories of the past. A historian must, of course, be committed to the supreme value of concrete empirical data. He must insist that all models and theories be constantly confronted with the richness of concrete experience. He must be committed to the view that concrete facts may kill a given generalization or challenge the universal claims of a given model. He is convinced that only a confrontation with concrete experience can deepen and broaden our ideas. One might say that one of the main tasks of history is not so much to confirm given theories and models as to see whether they are falsifiable. It is, however, infinitely easier to falsify than to verify. The notion that certain sweeping hypotheses have been "scientifically" verified by certain types and quanta of facts will be infinitely more convincing to those who accept the models to begin with than those who do not. The fact that one can find a high correlation between two series of statistics may represent a very local relationship and not a law of history. There still remains no royal road to verification in all the larger matters of human history and we have not lifted ourselves out of the dilemmas which confronted our intellectual ancestors. If their intellectual gropings and fumblings inevitably and exclusively reflected the interests of the ruling class, the structures of their culture, or their childhood traumas, then so do ours (as many of the young so vehemently insist).

The notion that the ideas and preoccupations of those who write and leave records are most often of concern only to priests, monks, religious visionaries, scholars, intellectuals, or effete ruling classes is based on what might be called a patronizing humanitarianism. In this view, masses who live in poverty should be properly concerned only with their poverty. Yet as Lévi-Strauss and others have pointed out, the Australian primitives who subsisted on berries and grubs developed vast mythologies, theologies, and cosmologies. There may be certain kinds of scholastic developments and certain kinds of learned jargon which are truly the preserve of the learned and inaccessible to the masses. This is by no means peculiar to traditional society. Both Marxist social science and American varieties of social science have developed precisely this type of scholasticism and learned jargon to an exaggerated degree,

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yet the practitioners of these scholasticisms remain firmly convinced that they are dealing with matters which are of universal concern. In short, the ideas of thinkers and intellectuals in the past must be taken seriously because they represent efforts quite as serious as our own to cope with the human condition in various times and places. Without assuming that these ideas are the embodiment of a *Weitgeist*, they nevertheless represent preoccupations which may reflect matters of concern even to the illiterate masses.

I have not, however, provided a justification for the particular concern with political and intellectual history in dealing with a non-Western culture in the face of the strong current within the profession to stress "objective" "social scientific" history. In his *Pensée sauvage* Lévi-Strauss states that the ultimate goal of the human sciences is "not to constitute but to dissolve man,"[7] that is, to dissolve him into the various impersonal structures, biological, societal, anthropological, and so forth, which make up his being. Within the French context this undoubtedly represents a reaction against certain forms of existential humanism with their strong stress on subjectivity and a strong reassertion of the pathos of objective scientism. However, in attempting this undertaking in the West, one does so in the face of a vast and profound literature concerned with "constituting man." Any effort to force Rousseau into someone's behaviorial scientific scheme must face a vast battery of Rousseau experts who know their Rousseau much better than the schematizer, while Jean Paul Sartre is quite capable of defending himself against efforts to fit him into someone's anthropological scheme.

In the case of China, however, the effort to establish the human reality of China across the centuries, of seeing Chinese responding consciously to the political and human situations which confront them, is a task which has still not advanced very far, at least in the West. This makes it far easier to impose our models and preestablished structures onto China. The Chinese will not contradict us because we know so little of what they have said. As John Habakkuk states in a paper submitted to the conference for which this essay was written, "Where the data are sparsest, one has to rely most heavily on reasoning based on a model." He might have added that where the data are sparsest, it becomes easiest to impose the model. China has already suffered far too much from premature structuralism. It is to the credit of Lévi-Strauss that he has on the whole refrained from attempting to impose totalizing structures on the so-called higher civilizations, perhaps because of

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his suspicion that cultures which occupy such vast areas of time and space do not in fact possess one all-embracing structure.

It may, of course, be urged that a good deal of Western sinology has concerned itself with Chinese thought, hence with the conscious life of the Chinese. In his *Pensée chinoise* Marcel Granet has in fact provided us with a kind of image of the timeless essence of Chinese thought. Written under the influence of Emile Durkheim's conception of "collective representations," it attempts to provide us with the "deep structures" of Chinese thought in Lévi-Strauss's sense. The latter has, in fact, acknowledged his debt to Granet. If there have been synchronic and diachronic tensions and conflicts within Chinese thought, Granet dismisses these as "secondary manifestations."

Now what Granet does—and he does it with consummate skill—is provide us with an account of a certain kind of Chinese cosmology which has indeed been a dominant and persistent strand of Chinese thought. It was, moreover, a brand of cosmology which was not displaced by any rival type of cosmology until modern times. Yet the fact remains that although this cosmology held the center of the stage in the former Han dynasty, in the latter Han dynasty there was a distinct reaction against it. There is the fact that the Confucius of the Analects, while seemingly accepting elements of this cosmology,[8] refuses to place it at the center of his concerns. In the thought of many of the so-called Neo-Taoists of the Wei-chin period it was quite marginal, while there are many aspects of the history of popular religion which bear no particular relationship to the cosmological scheme although efforts were made to fit them into the Procrustean bed. To Granet the range of possibilities covered by Chinese thought may seem to be "secondary manifestations." To the participants, the conflicts and issues may have been quite as fundamental as the issues dividing Lévi-Strauss from Sartre or French Marxists from American liberals even though it can be maintained that the latter both share important general notions in common. An intellectual history of China, as here conceived, would be concerned not simply with "Chinese thought" but with Chinese thinking and thinking within the framework of their historical situations. Similarly, a political history of China would involve not simply a reconstruction of the timeless institutional structures of the Chinese state (crucial as this undertaking may be) but a history of the living politics of the men involved in these structures. In-

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deed, the evolution of the structure itself can hardly be understood apart from the context of concrete political situations.

It is, of course, not my intention to anathemize any method of historic investigation. Any kind of research procedure which porvides us with new significant data is relevant to any interpretation of history. It is rather my intention to defend against prevailing fashion an interpretation of history which is still concerned with "constituting man" rather than "dissolving him."

To some this concern with man's conscious life may seem to be a manifestation of a peculiarly tender-minded view of the human situation. While not particularly concerned to establish credentials for tough-mindedness, it seems to me that a concern with conscious attitudes and activities has no necessary connection with any facilely optimistic view of man's destiny. One may indeed contend that conscious attitudes, orientations, and visions of reality have more often than not played a devastatingly tragic role in human affairs.

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1. Claude Lévi-Strauss, "Overture to le Cru et le Cuit," cited in "Structuralism," *Yale French Studies* (October 1966).

2. See M. Freedman, *Lineage Organization in Southeastern China* (London: Athlone Press, 1958).

3. In actuality the theories of history that stress vast impersonal forces do not really ascribe any truly creative role to the masses of mankind. The masses are simply the units of psychological energy through which the large structures and processes manifest themselves.

4. Frederic Wakeman, *Strangers at the Gate: Social Disorder in South China, 1839-1861* (Berkeley: University of California Press, 1966).

5. Maurice Merleau-Ponty, *Eloge de la philosophie et autres essais* (Paris: Gallimard, 1965), p. 325.

6. See chap. 9, "History and Dialectic," in C. Lévi-Strauss, *The Savage Mind* (Chicago: University of Chicago Press, 1966).

7. *Ibid.*, p. 247.

8. The question of how developed this cosmology had become in Confucius' time remains a matter of dispute but there is a tendency to interpret the statement in the Analects that T'ien tao (the way of heaven) was one of the matters which the master seldom discussed as a reference to this type of cosmology.

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PIERRE GOUBERT

Local History

We shall call local history that which concerns a village or a few villages, a small or middle-sized town (a large harbor or a capital is beyond the local scope), or a geographical area not greater than the common provincial unit (such as an English county, an Italian *contado*, a German *Land*, a French *bailiwick* or *pays*). Local history, which was practiced long ago with carefulness, zeal, and even pride, was later despised (especially in the nineteenth and the first half of the twentieth centuries) by the supporters of general history. But, since the middle of this century, local history has risen again and acquired new meaning; indeed, some even maintain that only local history can be true and sound.

For a long period—at least until the time when ideas circulated faster (in the eighteenth century) and when men began to move frequently and quickly (during the revolution of the railways in the nineteenth century)—the setting of most Europeans was the parish in the country or the small town and the surrounding district—that is, roughly speaking, the stretch of land covered in a day's walk, from ten kilometers to ten miles, or in a day's ride, about two or three times more. The same laws (local custom) prevailed in this place, as well as the same cultural habits (methods of cultivating the soil, quality of crops, shapes of tools, date of opening for meadows and woods), the same social and economic habits (date of hiring servants, date of weekly markets and quarterly and yearly fairs), the same seignorial rules, the same judicial and administrative area, and the same religious beliefs. Most people never went beyond the boundaries of their districts; their children remained there; a priest, a judge, a seigneur, a notary, and a market were within reach. The cultured part of the population moved about easily: prospective lawyers would take some of their degrees in

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the neighboring universities, future priests went to diocesan seminaries when they existed, nobles, when they did not travel far away because of a war, went to their parents' or their suzerains' castles situated outside the bailiwick, or sometimes outside the province itself. There were also vagrants and beggars, of course. But all of them felt they were first of all citizens of their native towns and provinces—from Dijon and Burgundy, from Amiens and Picardy, from Nimes and Languedoc, from Aix and Provence, from Saint-Brieuc and Brittany—and secondly they were French, though they always acknowledged themselves as the king's faithful and obedient subjects. Only small segments of society—the highest stratum and the lowest—could have felt no association with a particular region or place. The great financiers and captains, as well as the gangs of beggars along highways, were not purposely adrift from their homes; all of them knew (especially when the police questioned the beggars) what province, diocese, and parish they came from.

In such a confined life, the intellectual activities of the small minority dealt with either meditations on ancient texts, especially Roman and Greek, or with the history of the region, the region being understood as the land of the family. When they tried to produce histories—as histories were written before Voltaire's *Essai sur les Moeurs*—antiquities, particularities, and the famous exploits of the city or province were emphasized. By the end of the sixteenth century, provincial histories—which were merely lists of noble families, castles, fiefs, abbeys, and cathedral chapters—or histories of towns and cities—which were enumerations of charters, privileges, famous people, and gossip about the writer's native town—were commonplace. Later, the strongest institutions in the provinces (the Estates, the *parlements*), because of their corporate feelings, and as a way of protecting their interests, published their histories. Monks dedicated to research (for example, the Benedictines) often decided to gather and publish a corpus of documents regarding their own provinces; and such work was frequently carefully done.[1] These generally contained legislative or administrative acts, the founding charters, feudal documents, and other evidences concerning the great noble families, the pious priests, and abbots who had been influential. They were sometimes apologies, nearly hagiographies. But they cannot be dismissed, even today. They often refer to the text of lost documents and give the only extant version of them.

The nineteenth century, at least in France, was the golden age

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of local history. Following and enlarging upon the example given by provincial academies in the second half of the eighteenth century, many "societies" that called themselves "scholarly" or learned (and occasionally were so) appeared on the scene, existing sometimes for only a few years. Their members were typical elements of bourgeois society: magistrates, notaries, priests, spinsters, *rentiers*, a few teachers, minor nobility. Among thousands of papers and hundreds of volumes issued in France from the middle of the nineteenth century, a tenth is perhaps worth glancing through, a hundredth worth keeping.[2] This "petit bourgeois" social science would profit from serious sociological and psychological analysis. The weakness of many of these pseudohistorical works partly explains the contempt with which professional historians of the early twentieth century regarded local history—a jumble of chance genealogies, usurped glories, proofless assertions.

Professional historians disdained such local history also because of their own conception of what they chose to see as "general" history. General history was political, military, diplomatic, administrative, and ecclesiastical. Studying the state involved a study of statesmen; studying war permitted a study of the military feats of generals; studying foreign relations involved the publication of ambassadors' memoirs; studying religion led to a recounting of the achievements of popes and bishops, generally holy and pious; studying administrative history (written from the records of offices located in Paris) was represented as being the history of a whole people. A retrospective psychoanalysis of the historians of the nineteenth century would probably reveal that many of the historians who chose to write about monarchy more or less identified themselves with the monarch, that the historian of a particular minister sometimes imagined he was himself governing the country. An elementary Freudian interpretation could explain many of the curious histories that were published.

Yet, the first serious attempts at local history were made by historians and intellectuals who rank with the best. They understood that a thesis or interpretation, however ingenious, needs to be supported by precise facts; precise facts have a space as well as a time dimension. Thus, Sébastien Vauban, pleading for fiscal reform, used the precise examples of the Vézelay election, his native *pays*, and Normandy, where he had often been.[3] Thus Messance, a demographer,[4] rightly opposing the thesis of French depopulation so brilliantly advanced by intellectuals as famous

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though unqualified as demographers as Montesquieu and Voltaire, proved with a strictly regional analysis of numerous parishes in the Lyonnais, Auvergne, and Upper Normandy that the French population had risen markedly between the end of the seventeenth century and the end of the eighteenth. Alexis de Tocqueville, with his great design, which was to understand at the same time the dying Ancien Régime and the French Revolution, used the provincial record offices, particularly in Tours, and illustrated with actual examples the accomplishments of the states of Languedoc.[5] But can a historical form depend upon the work of men of genius?

Alphonse Feillet's technique was more common and more commonly used. Feillet, an often-neglected historian of the misery in France at the time of the Fronde,[6] was a contemporary of Napoléon III. He believed that the French people did not sufficiently know the misdoings of "legitimate" Bourbon monarchies. To support his rather simple thesis, having to do with the misery of the French before, during, and after the Fronde, Feillet used memoirs, letters, monographs, and essays published in large numbers in the nineteenth century: one could always find a French village where somebody was dying of the plague or malnutrition, or where soldiers' violence had created massive disorder in every month between 1640 and 1660. A careful reading of the same documents by a historian who wished to prove the opposite would certainly have been equally compelling. It is quite possible to prove that France was flourishing or that it was miserable during Louis XIII's reign: one needs only to choose one's evidence carefully in the sea of published local histories.

The Feillet method (though Feillet never thought of founding a method; this was still a time when few laid claim to being concerned with methodology) has in fact become very widely used. Any thesis, if it is brilliant, unexpected, or paradoxical, may be supported by selected examples chosen from old scholarly studies of different provinces. History becomes a game where the guiltless amateurs of local history provide others with materials they find useful.

Only in the last twenty years has a new kind of local history become possible. The return to the unexploited archives of a given region and a given period was brought back into fashion by historians who generally were not born in the region studied and were not therefore expressing filial piety. This new trend derived from dissatisfaction with current historical methods, and a concern to

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establish new kinds of historical questions. Historians of the preceding generation were overwhelmingly preoccupied with the problems of the upper classes. To use French examples, it would be correct to say that the old school was interested in lawmakers and not in law enforcement, in those who governed and not in the governed, in the clergy and not in the faithful, in the memoirs of men of letters describing their countries and not in the realities of the country itself. The return to local history proceeds from a new interest in social history—that is, the history of a whole society, not only the happy few who governed it, judged it, ground it down, or taught it—the history of groups of men, sometimes called orders, classes, *états,* who lived together. This was history as keenly interested in the bodies and minds of the many as in the world-wide plans or profound thoughts of the few; as concerned with the history of bread, oil, and wine as with the history of corporate statutes and military rules. But a history that undertakes all aspects of human life in all classes of men meets first a major obstacle: numbers.

It is not too difficult to study thirty French intendants or twenty ambassadors; trying to study the hundreds of thousands of townsmen and millions of countrymen in all aspects of their lives presents insuperable difficulties. Historians concerned with such matters do not complain of the lack of documents but of their number. Lacking any adequate sampling techniques, and given the state of the archives, historians tried to limit their difficulties by restricting their gaze to a particular geographic region whose records were well gathered and could be analyzed by one man working alone. Lucien Febvre, as early as 1911, did this for the Franche-Comté; Gaston Roupnel did the same ten years later for the Dijon region.[7] These early works went unnoticed, probably because they were too strictly regional. In the French universities of that time (and even of the years following), such history was thought too limited. But these pioneers soon had followers, and they were soon joined by others, so many others that a sort of overproduction of regional history, at least in France, is a real threat today.

Why were these regional monographs important? They established certain proofs, limited in some instances, but proofs nonetheless; their statistics, compiled with a safety-margin, challenged some of the "general" ideas, prejudices, and approximations that had held sway in the absence of more precise investigation.

A few examples may serve to suggest what has been achieved.

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It had been common for a long time (and it is still too often common) to oppose unconditionally the nobility of the "sword" to the nobility of the "robe," that is to say, a hereditary nobility to a nobility conferred by appointment to certain important judicial functions. The judges of the French *parlements* were sometimes characterized as bourgeois. The careful thesis of Jean Meyer on the Breton nobility in the eighteenth century[8] proved irrefutably that there was not a single bourgeois and very few former bourgeois in the *Parlement* of Brittany, and that the difference between nobility of the sword and nobility of the robe was nonexistent. The members of the Breton *Parlement* were the descendants of the oldest noble families that had dispensed justice in their province. Obviously, what holds for Brittany may not be equally true of other provinces; the historian's task is to make as painstaking a study of the membership of these other French provincial *parlements* as has been made in Brittany.

To take a very different example, the growth of the seignorial system (which the Marxists call feudal) in France seemed to be quite common; an edict by Louis XIV was sometimes cited to show how the last allodial tenures fared under that king. R. Boutruche, in his study on allodial tenure in the Bordeaux region, has drawn our attention to the fact that completely free lands and peasants sometimes survived the Middle Ages.[9] In his book on the Lower Auvergne, Abel Poitrineau shows, on the basis of notarial documents, that the proportion of allodial tenures sometimes approached 50 per cent in many rural communities of eighteenth-century Auvergne.[10] This phenomenon will probably be found to have been common in other parts of the country as well (mainly in central, eastern, and southern France). The result of these many researches is the inescapable (but unanticipated) conclusion that a part of France during the Ancien Régime (and, by definition, the Middle Ages as well) did not undergo the seignorial system.

It was known that certain feudal customs such as *mortmain* had survived in remote parts of eastern France. Voltaire once spoke of the "serfs du Mont Jura"; the king in the eighteenth century liberated the last serfs who lived on the lands of which he was seigneur. New researches by Abel Poitrineau and Pierre de Saint-Jacob suggest that important groups of peasants subject to *mortmain* were to be found in central France; the study by Régine Robin on northern Burgundy (Auxois) reveals many more of them. It is possible that a fifth of the peasants of this small region lived under a system

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that was in fact a form of extenuated serfage.[11] Other discoveries of the same nature may be expected for eastern France. This phenomenon was neither known nor understood until minute studies carried on at the village level and seignioral level were undertaken.

Our image of rural France in this period has been substantially altered by these new insights. Thus, for example, the penetration of Indian corn into Aquitaine in the eighteenth century (an old assertion that even Fernand Braudel thought to repeat recently) is now dated to the seventeenth century. This knowledge is based wholly on the research of young historians who took an interest in the small corn markets of southeast France.[12] The idea, often repeated, that the southern half of France used a biennial rotation system in its corn fields was blasted when a diary of the journeys made by J. F. Henry de Richeprey, an agronomist and officer of the king, was published by a regional learned society.[13] It is now known that biennial rotation was not the rule; all types of rotations were used in the south (and an even greater variety in the west).

The very different attitudes of the peasants with respect to the clergy have been explained by referring to the rate of tithe that existed in a particular region. The tithe was very low in Lower Brittany (3 per cent) and very high in the southeast (from 10 to 12.5 per cent).[14] The clericalism of one region and the anticlericalism of the other has roots that are more ancient than is sometimes realized. One could go on endlessly to indicate what the new researches, local and rural, have contributed to altering our perceptions of Europe's past. It must be said that the researches have not been only those of French historians working on French materials; mention must be made of the renewal of English, Belgian, and Dutch history as a result of the local studies undertaken by W. G. Hoskins on Leicestershire, Joseph Ruwet on the *Herve pays*, and B. Slicher van Bath on Overijssel.[15]

The careful practice of local history and the multiplication of monographs on specific regions may lead much further; they may serve to destroy many of the general conceptions that once seemed so strong and were embodied in so many books, papers, and lectures. Thus, for example, the so-called "crisis" of the seventeenth century and the so-called "agricultural revolution" of the eighteenth century are certain to be reconsidered in the light of the material now being developed.

A whole book would probably be needed to bring out the nuances of, let alone to destroy the myth of the seventeenth-

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century "crisis," at least in France. The word "crisis" is itself unsuitable; *crise* in French means a violent but brief phenomenon; the use of the term in this sense seems to be disappearing. In my own study of Beauvaisis, I found no justification for the idea that there was a severe crisis in the period previous to the Fronde except for the incidence of epidemics and high mortality attendant on these, common in the seventeenth century (as well as the sixteenth). Population was rising, prices were going up, though slowly, the incomes of landowners were rising, and textile production had reached new levels in Beauvais in the 1630's. It was only in the second half of the century that conditions seemed to deteriorate. It is interesting that subsequent local studies in other areas suggested other patterns. In a thorough study of the northern textile cities, particularly Amiens, Pierre Deyon showed with impressive statistics the character of industrial growth in that area; in doing so, he supported my comments about the "blissful reign" of Louis XIII. After 1660, when Colbert was minister, textile activity came to life again; the industrial "crisis" was put off, and rightly, to the last years of Louis XIV's reign.[16] In his blunt, strange, but substantial thesis, René Baehrel discarded for rural Lower Provence the conception of there having been a crisis in the seventeenth century: he advanced the idea of a general improvement in conditions interrupted by a more difficult period every thirty years.[17] E. Le Roy Ladurie, in his researches focused on the neighboring region of Provence, postponed the coming of a depression till the years following the Holland war—that is, till the 1680's. His new research, especially his methodical use of documents relating to the ecclesiastical tithe, lead him to the conclusion that prosperity continued during Colbert's age, and reached a sort of climax, with greater agricultural production in the years between 1660 and 1680. In southern France the Fronde was never important; as there were no famines in the fifties and sixties, nearly the whole of the seventeenth century—at least until 1680 or 1690—was characterized by expansion.[18] Michel Morineau discovered through his studies in the Dutch record offices and libraries that the American treasures poured more lavishly into Europe in the second half of the seventeenth century than they did in the sixteenth century, so dear to E. J. Hamilton and P. Chaunu.[19] Thus, a series of local studies has led to a serious questioning of the oft-repeated thesis about the general crisis in the seventeenth century. If it occurred, it came at the very end of the century, at least in France. The subject, obvi-

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ously, is far from exhausted; new researches will perhaps bring new opinions and will revive old ones. It is quite certain, however, that without serious provincial monographs (written by professional historians and not by amateurs) a general revision of the kind suggested would not have been possible.

The "agricultural nonrevolution" associated with Michel Morineau involves a period even closer to us in time. A careful study of the accounts of ecclesiastical tithes and agricultural properties over several centuries allow Morineau to come to a striking set of conclusions: the same agricultural yields are observable in the Middle Ages, the sixteenth, eighteenth, and even nineteenth centuries where the same conditions of climate, political order, and good farming prevail; where, in some periods, outputs decline, there are always easily identifiable reasons: war, bad climatic conditions, social unrest, a closing down of markets. The eighteenth century generally combined fairly favorable conditions, but the outputs were not substantially higher than they had been; they merely reproduced the best outputs of earlier times. A young historian of Sicily, Maurice Aymard, using the same methods in a quite different place, maintains that the yields of good Sicilian corn fields are the same in the eighteenth century as in Cicero's time. An upper and unvarying limit of agricultural output would appear to have existed. No rapid growth can be observed before the nineteenth or the twentieth century. The changes that historians thought they were discovering were changes of short duration, or geographical variances between different regions.[20] Not all historians are ready to accept the rather startling theses propounded by Morineau. They try to discover other regional examples to refute him; often, they are more comparisons of the bad periods of the seventeenth century with the best periods of the eighteenth. The idea of a technological limit appropriate to most regions has certainly to be kept in mind. The slight rise and fall between a maximum and a minimum yield (the one and the other being proper to different regions) probably expresses what was true most of the time. The real agrarian revolution began to "take off" very late—nearly everywhere in the second half of the nineteenth century or even later; sometimes, even more recently.

A now very lively branch of history—historical demography—owes its development to the narrowest type of local history. This field was vegetating, with rumors and proofless theories as its staples, before historians and statisticians became interested in modest, neg-

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lected, and widespread documents: parish registers. Using parish registers from Gien (on the Loire River) and from several other places, Jean Meuvret pointed to the link between economic crises and demographic crises in northern France in the seventeenth century and to the weakening of these crises after the middle of the eighteenth. An examination of about thirty parishes in Beauvaisis was the first attempt to make a serious demographic analysis of a whole region. Louis Henry, using the register of Crulai (in Normandy), gave new impulse to the professional use of such data; soon afterwards, he laid down the well-defined rules of method (now unquestioned) that are appropriate for such study. Recognition of the value of parish documents (at a time when, in France, censuses were rare, not well known, and not easy to use) allowed studies to be made of fecundity, marriage, and infantile mortality, with a possibility of accuracy never previously attained. The growing monographic literature soon demonstrated that neither France nor Europe looked like either Crulai or Beauvaisis, but it allowed historians to raise a whole series of new questions: thus, for example, about the beginning of birth control procedures, about nursing practices, about mobility more generally. The problem of illegitimacy and sexual intercourse prior to marriage, with patterns so different as between France and England, and perhaps so typical of a kind of modern mentality,[21] could not have been examined before the arrival of good methodical censuses (1840 in France). Only the parish registers provide the key to all such problems for an earlier period; without them, such problems could not even be tackled.

The success of local and regional historical studies has been considerable in France. The reason for the success is not explicable simply in terms of the methods used. Rather, it must be seen how much of the inspiration for such study has come from the *Annales* school. That school, severely critical of traditional ideas and elitist prejudices, drew attention to new social groups and provided felicitous interdisciplinary associations between historians and scholars in economics, sociology, psychology, biology, and demography. A regeneration of historical studies, with new methods and ideas, was made possible by a new generation that had sufficient talent to make itself heard.

All success brings with it excess. As historical work in France is carried out mainly within universities, it tends to reproduce the

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faults of the universities. Soon, a servile imitation of innovation establishes a new tradition, or a negative and violent contestation which often ends in reinforcing the thing originally disputed. One can already foresee the birth of yet another novelty, which will owe much to contact between linguistics, psychoanalysis, and history[22] (just as soon as psychoanalysts and linguists agree to study archives, and historians agree to study linguistics and psychoanalysis). A brutal reaction, ideological and political, could also take place, throwing French historians back fifty years or more.

In the most recent period, large numbers of advanced students and young historians devoted themselves to parish monographs. For a year (for the certificate *mémoires de maîtrises*) or for several years (for the thesis of the *troisième cycle*, a kind of Ph.D.), they analyzed, using the archives, different aspects of life in one or several parishes, most frequently in the eighteenth century. That century left abundant archives, they were varied, and they were usually easy to read. The work of novices is generally worth about as much as their authors (and the professor who advised them) are worth. Most often, they simply confirmed what was already known. Some, however, did contain certain new ideas and provided unexpected information—thus, for example, on the early attitude toward birth control of the winegrowers of the Ile de France in the eighteenth century,[23] or on the composition of communities in the villages of Brie (to the east of Paris) in the same century,[24] new information has been provided. It had been thought that the communities of Brie were dominated by rich farm workers; one now finds that several were dominated by the poor, numerous *manouvriers*, who united against the rich peasants through the help of the Intendant General of Paris. Many such parish monographs are poor; they deserve to be forgotten; others are excellent.

Be that as it may, their multiplication approaches overproduction and presents at least three disadvantages. First, it is difficult to find them, read them, or make a synthesis, the more so as the majority of them are not published. Second, an advanced student or a fledgling historian is unlikely to have a sufficient competence to deal with the different elements necessary to good local analysis; law, institutions, economics, demography, sociology, religion are not subjects easily mastered. In the last analysis, there is always the danger of a return to amateurism. Third, even when the monograph is good, the description of an isolated village raises more

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problems than it resolves: Does the exact information provided have local, provincial, or general significance? To decide such questions, one would need to have other monographs on neighboring parishes as well. The questions and the demands are endless.

The solution, obviously, lies in a systematization of monographic study. In the demographic domain, this systematization is assured by the Société de Démographique Historique in collaboration with the Ecole Pratique des Hautes Etudes, the Institut National d'Etudes Démographiques, and a number of universities; the first two institutions assure publication of the best monographs (or, failing that, of at least a résumé of them). At Aix-en-Provence, the work of advanced students has been directed toward social analysis of the rural communities in the eighteenth century. At Caen, Pierre Chaunu follows the same systematized effort for Normandy. At the Centre de Recherches sur les Civilisations de l'Europe Moderne de Paris, Roland Mousnier has centered the work of his students around several themes: revolts of the seventeenth century, governmental staff of the Ancien Régime, Parisian society during the same period.[25] The difficulty is to achieve genuinely good collaborative publications.

Large provincial studies continue to be undertaken, and many are nearing publication. We expect studies on Anjou, Upper Normandy, the region south of Paris, Lorraine, Dauphiné, Provence, Toulouse, and Gascogne; others will follow somewhat later.[26] These major studies are the only means of verifying the validity of old ideas and propositions, or of discovering new problems and hypotheses. The new trend, in my view a good one, is to examine one or two problems in one or two regions. Thus, for example, the interesting and original work by François Lebrun on men and death in Anjou during the seventeenth and eighteenth centuries is awaited with great impatience. Two of my own best students have recently chosen to concentrate their efforts on the problem of marriage in Champagne and the problem of *metayage* in western France from the sixteenth to the nineteenth centuries. Large provincial studies, concentrating on one important problem, analyzed over a long period (a century or more)—this is perhaps the best course to take for those who wish to remain faithful to the idea of investigating local/provincial history.

I warmly thank my daughter Annie Gresle for her gentle assistance in the English language.

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LAWRENCE STONE

English and United States Local History

THE HISTORIOGRAPHY of local history in England in the nineteenth century closely parallels that in France, as described by Professor Goubert. Between 1680 and 1830 there was a remarkable output of county histories, so that by the accession of Queen Victoria most counties were the subject of at least one, and often several, stately folio volumes. In the 1840's there began a rapid proliferation of county archaeological and antiquarian societies, the hobby of landed gentry and clergy with an amateurish interest in the history of their locality. The culmination of this period of activity was the *Victoria County History* which began in 1898 with the intention of providing a scholarly factual history of every parish and town in England. The limitations of this undertaking are now well known, in particular its obsession with manorial descents and the history of the Established Church, and its neglect of the dissenting churches and of the social and economic life of the community. As originally designed, it was basically a history of the upper classes written by the upper classes for the upper classes. The most extravagant example of this state of mind was the history of the Warwickshire parish which solemnly recorded the transmission of the manor from hand to hand but totally failed to mention that at a certain point the village itself was deserted and disappeared.

Between 1920 and 1940 this great enterprise ground almost to a standstill as social changes in the countryside, notably the impoverishment of the gentry and clergy, eroded the basis of its support. For similar reasons the antiquarian societies also fell on hard times, although more and more of them turned their attention to the scholarly publication of local archives. Slowly, a great corpus of printed material was built up, ready for the local historians of

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the future. The first of the professional historians to use local history to solve national problems were the medievalists, who in the early twentieth century—even before the French had got to work—were establishing the broad outlines of land tenure and field systems in the English countryside. The turning point came, however, in the late 1940's. Local county record offices were established in almost every county, and began to gather in, sort, and catalogue the mass of public and private local archives which had hitherto been almost inaccessible to the scholar. The *Victoria County History* project took a new lease on life, more and more under the direction of professional historians, and displaying increased interest in social and economic history, education, and nonconformist sects—a shift of focus toward the masses of the poor and away from the landed elite. Soon afterwards there emerged what can best be described as the Leicester school of local history, centered around a newly created chair of local history at the University (then University College) of Leicester. The leading figures in this renaissance have been H. P. R. Finberg, W. G. Hoskins, Joan Thirsk, and Alan Everitt (all associated with Leicester) together with J. D. Chambers of Nottingham. This group seems to have owed little or nothing to the French school, although, like the French, its main interest has been in the sixteenth, seventeenth, and eighteenth centuries. This temporal concentration is largely because the source materials are rare and hard to interpret for the Middle Ages, while after 1830 local history tends to become urban history, which is something rather different. The subjects of scholarly investigation have been the traditional English ones of field systems, land tenure, landlord-tenant relationships, and agricultural improvements. This selection of problems is a sensible one, if it is agreed that what above all else distinguished England from the Continent in the early modern period was the disappearance of the peasantry, and the rise in agricultural productivity. This work has mostly been exquisitely small scale, either focusing on a single village or town, or else tackling one specific subject, such as enclosures. A good deal of the best and most important material has appeared in the form of articles rather than books.

The basis of the work has been detailed topographical studies of a microcosm, usually a village, employing a combination of historical records, personal inspection of the terrain, and the new tool of aerial photography, which English historians were among the very first to explore soon after the war. In some cases archaeo-

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logical excavation has been used, especially on medieval deserted villages and more recently on town dwellings and town churches. Examples are H. P. R. Finberg's work on Anglo-Saxon boundaries, M. Beresford's studies of deserted villages, and M. Biddle's current work on medieval Winchester. From these small-scale village studies there has gradually emerged some larger regional surveys of specific problems, the most notable being the *Agrarian History of England*, currently in progress under the editorship of Dr. Joan Thirsk. Regional studies are also beginning to appear on other topics, for example nineteenth-century education, agricultural economy, and social structure by Margaret Spufford of the Leicester school.

Thus whereas the French jumped right into broad regional studies without prior attempts at detailed topographical inquiry, the English built up slowly from individual examples, and are only now beginning to synthesize their material. As a result the French were the first in the field, where they set standards of the highest quality. The main English contribution as they move to a higher level of synthesis is likely to be a greater awareness of the economic diversity of subregions and smaller districts, which tend to get lost in the broader studies of larger regions favored by the French school.

One of the more curious features of the Leicester school, which is part of both its strength and weakness, has been its provincial and rural bias, which for some of its members takes the form of an overt hostility and suspicion of London, and a nostalgia for pre-industrial, pre-urban England before the nineteenth century. This engaging romanticism is in striking contrast to the hardboiled professionalism with which the modern French historians dissect a provincial society and economy.

In addition to this Leicester school with its well-defined set of problems, the famous "gentry controversy" of the 1950's stimulated a large quantity of local studies, carried on in many different centers, concerning the fortunes of the landed classes in the sixteenth and seventeenth centuries, which were designed to apply a set of general hypotheses to a local situation and to see how they worked. In the last few years there have also appeared a number of important politico-economic studies of towns such as Exeter, Leicester, and Newcastle, particularly in the seventeenth century. It is here that some of the most promising work is likely to be done in the future, particularly since the Leicester school is now moving heavily into

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this area under the leadership of Professor Everitt, who is working on Northampton. What is still lacking is a full-scale socioeconomic history of London during the early modern period, without which English local history remains a torso without a head, a Hamlet without the Prince of Denmark. At this time, however, two multi-volume studies of London are in preparation, which may ultimately go far toward filling this gap.

All this is very different from the massive assaults on every aspect of a region which are the glory of the French school. One reason is that documentation is poorer in England, which lacks notarial archives, and which has always been grossly underadministered compared to France. Another is that the English local historians have been too modest to aspire to the writing of *histoire totale,* and have lacked a simple tool for historical analysis such as the *structure/conjoncture* dichotomy around which the French have organized their researches. Lastly there is nothing in the English professional career-pattern like the *grande these* which is the main stimulus to force the production of the vast French undertakings. Consequently, although the English school of local history is already a most distinguished one, especially in the field of topography and agrarian history, it has not yet produced a study on the scale of Goubert's *Beauvaisis* or Le Roy Ladurie's *Languedoc.* Nor has it yet devoted as much attention to local subcultures and mental attitudes as it has to economic organization and agricultural output.[1]

In the United States, filio-pietism in the nineteenth and early twentieth centuries along the eastern seaboard led to the publication of a remarkable quantity of local records about the old colonies and their early settlers. But not much use was made of these materials by professional historians before the 1960's, when there suddenly emerged a distinctive New England school of local history, most of whose members were students of Professor Bernard Bailyn of Harvard. This school has so far exclusively concerned itself with New England in the seventeenth and eighteenth centuries, and its focus has been the township rather than either the county or the state, or a particular problem examined on a regional scale. Like the English school, it has begun with the microcosm of the village, although it has been deeply influenced by the recent work of the French school of historical demography, and its offshoot at Cambridge (England). This group of young scholars—J. Demos, P. Greven, K. Lockridge, Chilton Powell, M. Zuckerman—have all concerned themselves with what it is that distinguished life in

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colonial New England from life in contemporary England. They have identified the principal variables and have set to work to elucidate the complex and changing relationships between them: these variables are the family, with the rise and fall of patriarchy; population, with the shift from scarcity to relative abundance; land, with the shift from abundance to relative scarcity; and government, with the shifting conflicts between oligarchy and democracy, centralization and the dispersion of authority. They have identified the right targets, they have developed a sound methodology, and they have made considerable headway by discovering the evidence to solve the problems they have asked themselves. One of the many tasks of the 1970's will be to extend the number of the samples, and in particular to test the hypotheses which have been formulated for New England in the very different conditions of New Jersey, Pennsylvania, Virginia, and the Carolinas; work on this is just now being undertaken by Philip Greven, Robert Wells, and James Henretta.

References

1. In this review of the English literature I have been much assisted by the suggestions of Dr. Joan Thirsk. The value judgments, however, are my own.

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JOHN E. TALBOTT

The History of Education

HISTORIANS HAVE begun to stake new claims in the history of education. Over the past decade a number of pioneering books and articles have appeared on subjects whose long neglect now seems quite remarkable—from the Spanish universities under the Habsburgs to childrearing practices in colonial America. Despite the great diversity of themes and problems with which these studies are concerned, they share a similarity of approach. Nearly all seek, to use Bernard Bailyn's phrase, "to see education in its elaborate, intricate involvements with the rest of society."[1] This approach has opened new perspectives in a field historians had long ignored.

Not that the history of education has been neglected. The bibliography in the field is enormous. But it is lopsided, mainly concerned with "house history" and the ideas of pedagogical reformers. Countless histories of individual schools and universities have been published, describing aims, organization, faculty, curricula, finances, student life, and so forth; histories of pedagogical ideas have surveyed the views of leading theorists from Plato through Dewey. To be sure, great monuments of historical scholarship stand forth in the field; such studies as Hastings Rashdall's *The Universities of Europe in the Middle Ages* and Werner W. Jaeger's *Paideia* are not likely soon to be surpassed.

But to the contemporary historian much of the older literature seems inadequate—impressive in bulk but insubstantial, seldom addressed to the sorts of questions with which historical scholarship is now concerned. A good share of the institutional history has been the work of antiquaries and devoted alumni, who uncovered much valuable information but rarely sought to interpret it. With the professionalization of schoolteaching and the establishment of teachers' colleges educational history became nearly a separate discipline, isolated from the mainstream of historical study. The educationists

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who founded and sustained these institutions sought to give prospective schoolteachers a historical sense of mission, certainly a not unworthy aim. But in the hands of some of them educational history became a weapon against adversaries living and dead, a vindication of their own ideas and efforts in the struggle for public schooling; in the hands of others it became a whiggish chronicle, a quick guided tour of the past in search of the antecedents of contemporary educational institutions.

If historians were frequently heard to lament the inadequacies of traditional approaches to the history of education, they were slow to do anything about them. Only after the Second World War, when educational issues began to loom larger in the public consciousness, did they turn in any numbers to the subject. The deepening crisis in education, charted in issues of this journal, from the neutral-sounding "The Contemporary University: U.S.A." of 1964, to "The Embattled University" of 1970, is likely to accelerate this trend.

Trends in the professional study of history also encouraged the new interest. Chief among these, perhaps, was what might be called an increasing concern for the interrelatedness of past experience, brought on by the pervasive influence of social history, the emphasis on interdisciplinary approaches to the past, and the collapse of the internal boundaries that once delineated "areas" of historical study. Historians began to recognize that education touches upon nearly all aspects of a particular society. The historical study of education came to be seen not only as an end in itself but as a promising and hitherto neglected avenue of approach to an extremely broad range of problems.

New approaches to the history of education differ from the old primarily in the attention now being given to the interplay between education and society. But this is a very great difference indeed. What was once a narrow specialty is now seen to have such broad ramifications that it has become hopelessly ill-defined. For if the role of education in the historical process is to be understood, attention must be paid to the external influences that shape the educational arrangements a society has made; to the ways in which these external influences impinge upon each other at the same time they are acting upon education; and to the ways in which education itself influences the society. The social composition of an elite educational institution, for example, is the consequence of the interaction of economic factors, of patterns of social stratification, of the conscious political decisions of the established authorities, and so forth.

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It is one thing, however, to recognize all the influences that need to be taken into account in the study of a particular problem in the history of education. It is quite another thing to determine the relative weight that is to be assigned each of them, especially over long periods of time. Research is at an early stage, and has not moved far beyond an enumeration of influences. In some important areas, such as the study of literacy in pre- and early industrial societies, the collection of raw data has scarcely begun; only recently have statistics long available in manuscript form been published and useful manuscript sources rescued from neglect.[2] In advanced industrial societies, where statistics on literacy abound, nearly the entire adult population is formally literate. But these figures conceal the functional illiteracy that is one of the consequences of technological change, and about which very little is now known. The questions that need to be asked in the new educational history are only beginning to be clarified. Conceptual models for dealing with these questions have yet to be devised; the methodological controversies that have enlivened more developed areas of study have yet to take place.

Thus the history of education is an area of study whose potentialities are only beginning to be exploited and the inchoate state in which it now exists is precisely what makes it attractive. So varied are the purposes now brought to the history of education, so patchy is the present state of knowledge, that the field does not lend itself to systematic treatment. Nevertheless, an idea of present concerns and problems can be conveyed by tugging at a few strands in the network of relationships that bind education to society.

I

Conventional histories of education are filled with generalizations about relationships between education and social structure. It has been a common practice, for example, to attach a class label to an educational institution, which is then held to respond to the "needs" or "demands" of a particular social class. Who determines these needs, or whether, if such needs exist, the institution in fact responds to them, is left unclear. Moreover, such static descriptive statements, based on implicit assumptions about how the class system works, explain very little about the dynamics of the interaction between education and the structure of society. Nor do they allow for the possibility that cultural values and styles of education once

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presumably moored to a particular social class may drift loose from that class and become the common property of an entire society—in which case they are not particularly amenable to class analysis except in its crudest forms. It is hard to see how describing an American university education as "middle class" explains very much about either the American university or American society. To be sure, education and social class have been, and continue to be, intimately connected. But the complexity of the historical connections between them has only begun to receive the carefully nuanced analysis it requires.[3] One would expect to find a large number of aristocrats' sons in an institution labeled "aristocratic." But one would also find some people who were not the sons of aristocrats. Who were they? Furthermore, one might also find aristocrats' sons in fairly large numbers in institutions not traditionally associated with the aristocracy. What were they doing there? Detailed research has only recently begun on who actually received the education a particular society has offered, how this has changed, and what the causes and consequences of change have been.[4]

With more attention being paid to who actually got educated, historians are now beginning to see that relationships between education and social structure have often been different from what the providers of education intended. It is roughly true that, until very recently, the structure of education in most European countries had the effect of reinforcing class distinctions and reducing the flow of social mobility—and was often intentionally designed to do so. Different social classes received different kinds of education in different schools; the upper levels of education were the preserve of the upper classes, a means of maintaining their children in established social positions and of bolstering their own political and social authority. But attempts to make patterns of education conform to the pattern of society have often been frustrated, both by forces the established authorities have been unable to control and by changes in other sectors of society which they have promoted themselves. One example of the latter is an expansion of job opportunities. Lawrence Stone has shown that in early modern England, economic growth and the proliferation of the bureaucracy of the state triggered an educational expansion: "So great was the boom . . . that all classes above a certain level took their part,"[5]—a consequence not entirely welcome to the ruling class of a highly stratified society. Other forces, of which demographic change is one, need to be identified and assessed.[6]

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Patterns of social stratification affect the structure of education. But educational arrangements also turn back upon the structure of society and exert their own influences upon it: the relationship between education and social stratification is a two-way street. This process can be seen at work in the history of European secondary education. Elitist patterns that took shape during the sixteenth and seventeenth centuries, when the hereditary ruling classes sent their sons to secondary schools in increasing numbers, persisted well into modern times. Recent studies of the English public school have addressed themselves to some of the consequences of this persistence. Among the most important and far-reaching of these consequences was the preservation of the values and attitudes—and therefore the social ascendancy—of the aristocracy, in a fully industrialized and formally democratic society.[7]

Research into who actually got educated will lend a good deal more precision to statements about the historical role of education in the promotion of social mobility and in the maintenance of established social positions. Until recently, these have possessed all the rigor of the notorious generalizations about the rising middle classes. Such research should shed light on changes and continuities in the recruitment of elites, matters of particular concern to social and political historians. What has been the role of education in this process, in the long movement away from a society in which status was based on birth to a society in which status is increasingly based on achievement? What have been the social and political consequences of the paradoxical principle of the career open to talent, which holds that everyone should have an equal chance to become unequal? To what extent were traditional elites able to adjust themselves to the pressures for meritocratic standards of recruitment which emerged from the economic and political revolutions of the eighteenth century? To what extent did the implementation of such standards truly open the way for new men? For the upper levels of education, which prepared their clientele for elite positions, abundant evidence is available on the social origins of students over long periods of time. University matriculation registers, for example, are waiting to be tapped.

But it is not enough simply to describe with greater precision the role of education in the promotion of social mobility (or in the maintenance of established social positions). It also needs to be asked what the consequences of this form of mobility have been, what it has meant to the individuals who experienced it and the societies in

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which they lived. Such qualitative questions may be exceedingly difficult to answer.

The education of the lower classes presents the historian with equally difficult problems. What influences have primary schools exerted on the values and attitudes of their clientele? Recent studies of elite institutions offer persuasive evidence of the ways in which education acts upon social structure through the medium of values and attitudes. But for lower levels of education, the kinds of literary evidence that permit one to generalize about the gentlemanly lifestyle of the public school, or the bourgeois ethos of the lycée, rarely exist. So far, historical studies of the impact of popular education on values and attitudes have been mainly concerned with such public issues as the promotion of nationalism and nation building—as in the case of the Third French Republic, whose founders quite consciously undertook a sweeping reform and extension of a state-supported system of primary education in order to provide a new regime with republicans. Comparatively little is known about the role of the school in shaping attitudes toward more ostensibly private matters, such as sex, or toward such divisive questions as social class. Analysis of the content of textbooks would at least suggest what attitudes the authorities sought to inculcate, though the degree to which they succeeded is quite another question.[8]

Attention to the social consequences that educational arrangements have produced, apart from what their designers intended, should help put to rest the largely speculative leaps of the kind which assume an exact correspondence between the structure of a society and the structure of its education. Indeed, given the extraordinarily high incidence of anachronistic features that educational arrangements exhibit (such as the persistence of classical studies in the West), it is hard to see how such a direct correspondence could ever have been drawn. Instead, historical relationships between education and social structure, as one sociologist has perceptively remarked, "are various, involve structural discontinuities and are singularly lacking in symmetry."[9]

Nevertheless, the sons of the rich are usually better educated (or spend more time in school) than the sons of the poor. As soon as education began to confer social, economic, or political benefits, the question of who should be educated became a source of bitter controversy. Some of the involvements of education in politics have received considerable attention: the intervention of the state in the provision of popular education has been one traditional area of con-

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cern. State intervention followed on centuries of debate about the wisdom of providing widespread education; seldom has a question been agonized over so long and settled so swiftly. The arguments for and against popular education, the activities of certain reform groups, the legislative aspects of reform, the church-state struggle, have all been treated in a number of studies. These questions fall within the traditional preoccupations of political history. But a vast amount of territory remains to be explored, and older interpretations need to be reexamined.

Older studies, for example, regarded the extension of popular education as an aspect of the process of democratization, a necessary consequence of the implications of liberal political philosophy. More recent work has held that the decisive motive in the drive for public schooling was social control of the lower classes in an industrializing and urbanizing society.[10]

But both interpretations are mainly concerned with the attitudes of the upper-class proponents of widespread schooling; they stress the intentions of the reformers, not the consequences of the reforms. Very little is known about the attitudes toward education of the people whom the upper classes quarreled over. Popular education needs to be studied "from below," and several works have opened the way. E. P. Thompson, for example, has shown how an eagerness for learning and an enthusiasm for the printed word were important elements in the radical culture of the English working class.[11] Inquiry is now moving beyond the confines of the politically-conscious elements of that class. What were the attitudes toward education of the unskilled and illiterate laborers who poured into the factories with their wives and children in the early stages of industrialization? Literary evidence is likely to yield very few answers; such evidence as does exist is likely to be testimony from men who were not themselves workers. An investigation of this kind must rely on indirect evidence: census records, school attendance records, the reports of factory inspectors, and so forth; new methods must be added to those already devised for dealing with the inarticulate.

Traditional governing elites, from their point of view, at least, had reason to fear the possible consequences of widespread literacy. To be sure, there existed conservative arguments in support of popular instruction. In Protestant countries, Christian duty seemed to require that the people be enabled to read the Bible; the idea that popular literacy was one more means of teaching the lowly respect for their betters and resignation to their lot bolstered the

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moral and religious arguments in its favor. But once people had been taught to read, it was nearly impossible to control what they read, without resort to the extraordinary measures which only twentieth-century dictatorships have been willing, or able, to undertake. Events of our own times provide abundant evidence that education can influence political behavior and the structure of politics in ways that the established authorities by no means intend. This aspect of the relationship between politics and education offers many promising lines of historical inquiry.

In recent studies of revolution, for example, attention has been given to the conditions which produce that ubiquitous revolutionary figure, the alienated intellectual. An oversupply of overeducated and underemployed men seems to be a common plight of countries in the early stages of development.[12] These conditions existed in both seventeenth-century England and eighteenth-century France. In both countries an expansion of enrollments at the upper levels of education produced too many educated men seeking too few places, frustrated in their ambitions and ready to turn against a society that had no use for their talents. All that was needed to create an extremely dangerous situation for the established authorities was an ideology which enabled personal grievances to be elevated into opposition to the regime: Puritanism in the case of England; a radical version of the Enlightenment in the case of France.[13]

If historians have begun to hammer out answers to important questions concerning the relationship between education and politics, in the equally significant area of education's links with the economy they are just beginning their work. Economists since Adam Smith have been interested in the relationship of education to the economy, and particularly to economic development; in the last decade the economics of education has become a vigorous subdiscipline. But historians have their own contribution to make, especially since the vexing question of the ways in which education has influenced economic growth demands historical treatment. As David McClelland has put it, "Did increases in educational investment precede rapid rates of economic growth, or were rapid increases in wealth followed by increased spending on education? Or did both occur together? These are the critical questions of social dynamics that cry out for an answer."[14] Historians are just starting to attempt to break the vicious circle in which such questions have been enclosed.

Take, for example, the problem of literacy—a topic which itself

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is only beginning to be investigated systematically. R. S. Schofield has remarked, "Today literacy is considered to be a necessary precondition for economic development; but the historian might well ask himself whether this was so in England at the end of the eighteenth century."[15] It would be plausible to argue that the relatively high rate of literacy that had long prevailed in England had much to do with that country's becoming the first industrial power. But on closer examination it is far from clear how literacy and schooling have contributed to rapid growth, especially in the early stages. In the first decades of industrialization, the factory system put no premium on even low-level intellectual skills. Whatever relationships existed between widespread literacy and early industrial development must have been quite roundabout. In one of the best treatments of this problem, Ronald Dore has shown that what was actually learned in school mattered less than the discipline involved in learning anything at all:

> But what does widespread literacy do for a developing country? At the very least it constitutes a training in being trained. The man who has in childhood submitted to some process of disciplined and conscious learning is more likely to respond to further training, be it in a conscript army, in a factory, or at lectures arranged by his village agricultural association. And such training can be more precise and efficient, and more nationally standardized, if the written word can be used to supplement the spoken.[16]

Directing his attention to a higher level of training, David Landes has recently argued that the links between technical and scientific education on the one hand and economic development on the other are much more direct than the links between literacy and development.[17] Certainly, the prima facie evidence in the classic comparison between the sluggishness of the British and the explosiveness of the German economies in the late nineteenth century, when industrial processes came increasingly to depend upon scientific innovation, would appear to support Landes's case: German scientific education was undoubtedly superior to British, and German entrepreneurs were more willing to hire and to heed the advice of graduates of scientific and technical institutes than were their British counterparts. But too little is now known about scientific education in the industrial age; historians of science have so far given more attention to the early modern period. When work in progress on scientific education in later times appears, it may well complicate, even if it does not substantially modify, the picture Landes presents.[18]

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Such studies, which define education as a process that takes place in specialized institutions, are likely to remain at the center of attention in historical writing. Nevertheless, any definition of education must be broad enough to include learning experiences which take place outside the framework of formal institutions, particularly within the family, whose role in the educational process remains of primary importance. But historical research on the family is now at a rudimentary stage. Very little is known, for example, about the ways in which responsibility for education after the earliest years of childhood shifted over a period of centuries, from the family to specialized institutions, such as the apprenticeship system and schools. Nor have we discovered much about the interaction between changes in the structure of the family and changes in the structure of education, or about how these changes have differed from class to class and among various levels of education. Did changes in family structure make formal educational institutions increasingly important agencies of socialization, or did pressures outside the family, from government or from social and religious institutions, provide the impetus for this shift in educational responsibilities? What have been the social and psychological consequences of these changes? How has submission to the discipline of the school altered the experience of childhood and affected patterns of adult behavior? Only in the last decade have such questions begun to receive the attention they deserve.[19]

II

If the exploration of "the involvements of education with the rest of society" is the new credo, it is a credo not without its own ambiguities and difficulties. The phrase can be interpreted in a variety of ways. It has been employed in a specific critical sense, to suggest the inadequacies of the history of education, old style, without meaning to lay down a program for the new. It has been used superficially, to dress up straightforward descriptions of educational institutions hardly different from older institutional histories.

More significantly, the phrase also lends itself to a quasi-functionalist interpretation which may distort the role of education in the historical process. This interpretation assumes that everything which may be identified with education responds to or fulfills the needs of society; that the structure of an educational system is merely a reflection of the class structure; that the pace of change in education

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is roughly equal to, indeed responds to, the pace of other changes in the larger society. First of all, it is never easy to decide what constitutes "society," the abstract entity to which education responds. Moreover, the functionalist view runs afoul of empirical evidence which suggests that the pace of change in education has often been widely at variance with the pace of social change. And this view is hard-pressed to allow for the anomalies and anachronisms so frequently found in educational systems. The relationship between change in an educational system and changes in the society of which it is a part is certainly one of the most important and least understood problems confronting the historian of education. An explanatory model which could be applied to this relationship would be an extremely useful tool, but for all its compelling simplicity—indeed, because of it—the functionalist approach is inadequate.[20]

The new credo may also be interpreted too broadly. An undiscriminating concern with relationships between education and society can lead to an emphasis on certain aspects of the role of education in the historical process at the expense of others. If the historical study of education is too preoccupied with relationships and interconnections, it may slight certain problems internal to the process of education itself, problems which may not be very satisfactorily explained in terms of external influences. Along with our attempts to understand how the larger society influences education, perhaps we need to understand the ways it does not.

For this very reason the study of educational institutions remains important—but what is needed is institutional history in a new key. The new studies should indeed take into account the larger social context in which educational institutions are located, but their viewpoints should be from the inside looking out. Only in this way are we likely to understand such matters as the consequences of educational reform as opposed to the intentions of reformers, or such significant topics in intellectual history as the influence which institutional settings exercise on patterns of thought and intellectual creativity.

Sheldon Rothblatt has attacked some of the foregoing problems in his recent book on nineteenth-century Cambridge, an important example of the new institutional history.[21] He argues that two historiographical traditions can be traced in the writing of English university history. The Whig interpretation assumed that university history could be written as an extension of political history: the ancient universities were seen as pliant tools of the Georgian

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Establishment. Unable and unwilling to respond to the challenge of industrialism, Oxbridge was forced into the modern world only by pressure administered from the outside, in the form of investigations by royal commissions. Of course the Whig version applauded these nineteenth-century changes. A second historiographical tradition, the class-conflict interpretation, holds that "the function of the university is to serve whichever social class is in power." This view reverses the Whig judgment: it does not regard the nineteenth-century reforms as progressive but as merely the transfer of control of the universities from one class to another. Whatever their respective merits, Rothblatt argues, both the Whig and the class-conflict versions have assumed much too close a fit between society's wishes and the response of educational institutions to them. Especially in a pluralistic society, "it is entirely possible that the university and society will be in subtle and complex states of disagreement as well as agreement with one another, that the direction of university change may not be completely obvious, that surprises will occur."[22] Rothblatt elaborates this thesis by showing how the reform of Cambridge, though quickened by external pressures, sprang largely from within the university, from the reformulation of donnish traditions which had very little to do with either the presumed needs of an industrial society or the "demands" of a rising middle class, and which in fact set itself against them.

The new institutional history is valuable not only as an illustration of the dangers in interpreting the new credo too broadly. There are other good reasons for maintaining institutional history amid the central concerns of the history of education. Despite the wealth of old-style studies of institutions, little is known about most of the problems with which contemporary historians are now engaged. Modeled on the constitutional history that dominated nineteenth-century scholarship, the older studies were preoccupied with formal structures; contemporary historical writing, on the other hand, might be characterized as mainly concerned with processes, with relationships of power and influence and social interaction that may have been widely at variance with the dictates of formal institutional structures. There are many histories of individual American universities, for example, but there is not even a handful of trans-institutional, comparative studies of the caliber of Laurence R. Veysey's *The Emergence of the American University*.[23] The standard history of the French universities, published at the end of the nineteenth century, is surely not the last word that can be written on the sub-

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ject.[24] And the histories of many other important educational institutions remain to be written.

Moreover, the study of universities is an ideal theater for historians interested in problems of the *longue durée*. For universities are one of the oldest forms of corporate organization in the West. Few institutions have been at once so fragile and so durable; few have been altered so radically, both internally and in their relationship to the larger society, adding new purposes, allowing others to lapse, and managing to maintain some of those for which they were originally founded. And perhaps because their existence and purpose presuppose an acute sense of the past, universities are rich repositories of information about themselves.

No scholar working alone can expect to take full advantage of these vast sources, nor can he exploit on his own that unique opportunity for the investigation of long-term problems offered by the university as a subject of study. The new institutional history demands collaborative efforts which will press into service the methods of several disciplines as well as the computer.

One such project, under the direction of Allan Bullock and T. H. Aston, is concerned with the social history of Oxford University from earliest times to the present. Another collaborative study, a statistical survey of universities in the West, is under way at the Shelby Cullom Davis Center for Historical Studies of Princeton University. A major goal of the project is to explore the cyclical patterns of expanding and contracting enrollments in Western universities, a phenomenon whose causes are not understood. Universities in England, Spain, and Germany exhibited similar patterns of rising enrollments in the sixteenth and early seventeenth centuries, of rapid decline beginning in the mid-seventeenth century, and of stagnation throughout the eighteenth. English and German enrollments again rose sharply in the nineteenth century. Was there a general decline in university matriculation throughout the West between about 1650 and 1800? If so, what were its causes, and what were its social, cultural, economic, and political consequences? Why did expansion resume in the nineteenth century? How did these cycles affect the pace and character of modernization in the West?

The Davis Center project is also concerned with two other problems: one, patterns in the relationship of university education to social mobility and to recruitment for professional, political, and administrative elites; two, the role of universities as transmitters of culture. The latter problem will consider education both as an

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intellectual and a socializing process and will ask how the structure, composition, and intellectual activities of the faculty changed over time—how and why the university was eventually able to become the critic of society as well as its servant.

Such collaborative enterprises can make significant contributions to our knowledge of the internal history of universities. But each study will eventually have to face the general problem with which much of this essay has been concerned: the establishment of cause-and-effect relationships between changes in education and changes in other sectors of society. This task, though the most difficult, may also prove the most rewarding.

III

Why has the history of education undergone such extreme change in recent years? The resurgence of interest is in part a consequence of the troubled state of contemporary education. New questions shaped by the dilemmas of our own times require new approaches to the past. The older historiography was found inadequate both because it was too narrow and inward-looking, and because it was ignorant of some essential facts concerning education itself. Long untouched by the great changes that have overtaken historical research in this century, the history of education became one of the last refuges of the Whig interpretation. So long as its practitioners were mainly concerned with searching the past for the antecedents of their own contemporary institutions, they could believe that education in the West had followed an upward linear progression. We now find this view hard to accept. We know, for example, that periods in which formal instruction was fairly widespread have been followed by periods in which it was restricted to small groups. The older historiography could neither accommodate such findings nor answer the questions they raise. Their exploration requires new modes of analysis. To pursue our example, it is clear that any satisfactory explanation of these expansions and contractions will have to take into account changes in demographic patterns and in the family, in the economy, the social structure, and the political system, in beliefs about the nature and purpose of human life. And it will also have to be recognized that education has turned back upon these influences in subtle and complex ways, working changes on its own.

Such a task is clearly beyond the old-style historian of education

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and the old-style historiography. But the demands of the task will not be satisfied by a new historiography of education as such. It may be doubted whether education, a process so deeply entangled in the life of an entire society, deserves to be called an "area of study" at all. Surely there can be little justification for making education a particular genre of historical scholarship. The history of education touches upon all the varieties of history. It is a task for the generalist, who must bring to the study of education a thorough knowledge of the society of which it is a part.

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FRANÇOIS FURET

Quantitative History

QUANTITATIVE HISTORY is fashionable just now both in Europe and in the United States. Since the 1930's historical research has been making rapidly increasing use of quantitative sources and of calculation and quantification procedures. But like all fashionable phrases, "quantitative history" has come to be used so sweepingly that it covers almost everything, from critical use of the simple enumerations of seventeenth-century political arithmeticians, to systematic application of mathematical models in the reconstruction of the past. Sometimes quantitative history refers to a type of source, sometimes to a type of procedure; always, in some way or other, explicit or not, to a type of conceptualization of the past. It seems to me that if one goes from the general to the particular and tries to pinpoint the specific nature of historical knowledge in relation to the other social sciences, one can distinguish three groups of problems relating to quantitative history.

(1) The first group concerns the methods of treating the data: problems to do with the formation of different families of data, the geographical unity of each family, and its internal subdivisions; with correlations between different series; with the values, in relation to the data, of different models of statistical analysis; with the interpretation of statistical relationships; and so on.

These problems belong to the *technology* of research in the social sciences. It is true they may also include questions of methodology. For ultimately they are bound to raise the problem of whether, and to what extent, historical or sociological knowledge is compatible with, or can be dealt with exhaustively by, mathematical conceptualization of a probabilistic kind. But neither the technical nor the theoretical debate is specific to history: both arise in connection with all the social sciences. In this respect quantitative history is no different from, for example, what is now called "em-

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pirical sociology," which in this context is simply contemporary quantitative history.

(2) Quantitative history also refers, at least in France, to the aims and researches of certain economic historians,[1] who attempt to turn history into a kind of retrospective econometrics, or in other words, on the basis of modern national accounting, to fill in all the columns of an imaginary input-output table for past centuries. The champions of this econometric history advocate total and systematic quantification, in their view indispensable both for the elimination of arbitrariness in selecting data and for the use of mathematical models in their processing. This processing is based on the concept of general equilibrium as imported into economic history from political economy.

According to this argument, genuine quantitative history would be the result of a two-fold reduction of history: first, at least provisionally, the reduction of its field to economics, and secondly the reduction of its descriptive and interpretative system to the one worked out by the most rigorously constituted of the social sciences today, political economy. The same analysis could be applied to demography and demographic history: here again a conceptually constituted science indicates the data and supplies the methods for a particular historical discipline, the latter thus becoming a sort of by-product of the other discipline, whose questions and concepts it merely transposes into the past.

Of course, there have to be data for the past just as for the present; or at least it has to be possible to work them out with a sufficient degree of accuracy, or to reconstruct or extrapolate them. This necessity sets the first limit to the complete quantification of historical data. Complete quantification, even if possible at all before the nineteenth century, could not go back beyond the introduction of the statistical or proto-statistical recording of data, which coincides with the centralization of the great European monarchies. But history did not begin with William Petty or Sébastien Vauban.

Moreover, there is no reason why the historian should agree, even provisionally, to have his field of research reduced to economics or demography. There are two alternatives. Either history is only the study of a previously determined, limited sector of the past, into which mathematical models established by certain social sciences are imported in order to be tested. In which case, we come back again to contemporary political economy, which seems to

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me the only one of the social sciences with such models at its disposal. History then becomes nothing more than an additional field of data. Or—the second alternative—one takes history in the widest sense, that is, as a discipline not strictly reducible to a set of concepts and with countless different levels of analysis, and then addresses oneself to describing these levels and establishing simple statistical connections between them on the basis of hypotheses which, whether original or borrowed, depend on the intuition of the researcher.

(3) This is why, even if one qualifies history as "quantitative," one cannot escape what is the specific object of historical research: the study of time, of the diachronic dimension of phenomena. But, looked at from this point of view, quantitative history's most general and elementary object is to form historical fact into temporal series of homogeneous and comparable units, so that their evolution can be measured in terms of fixed intervals, usually years. This fundamental and logical operation constitutes what Pierre Chaunu[2] has called "serial history," a necessary though not sufficient condition of strictly quantitative history as defined above. For serial history offers the conclusive advantage, from the scientific point of view, of substituting for the elusive "event" of positivist history the regular repetition of data selected or constructed by reason of their comparability. It does not, however, claim to give an exhaustive account of the whole body of evidence, nor to be an over-all system of interpretation, nor to be a mathematical formulation. On the contrary, the division of historical reality into series leaves the historian confronted with his material broken down into different levels and subsystems, among which he is at liberty to suggest internal relationships if he chooses.

Defined in this way, quantitative and serial history emerge as at once connected with and distinct from each other. But they share an elementary basis in that both substitute the series for the event, both make a construction from historical data in terms of probabilistic analysis. To the classic question "What is a historical fact?" they both give a new answer which transforms the historian's raw material—time. It is about this internal transformation that I should like to put forward a few ideas.

To avoid any misunderstanding, let me say at once that this essay does not set out to prescribe quantitative history as the only kind permissible. During the last ten or twenty years serial history has turned out to be one of the most fertile approaches in the

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advancement of historical knowledge. It has also the immense advantage of introducing into the ancient discipline of history a rigor and efficiency superior to those of qualitative methodology. But it is nevertheless true that there are important sectors of historical reality which it is by nature unable to treat or even to approach, either for circumstantial reasons such as irremediable lack of data, or for fundamental reasons such as the irreducibly qualitative nature of the phenomenon concerned. This explains why, for example, historians of antiquity, who work with data very discontinuous in time, or specialists in intellectual biography, concentrating particularly on what is unique and incomparable in creativeness, are usually less attracted by serial history than, say, historians of the industrial take-off of modern Europe.

But the fact that serial history has limits—a subject which might be discussed on another occasion—does not excuse intellectual indolence or absolve the historian from reflecting on the transformation which has taken place in his knowledge. Because of serial history he is now confronted with a new panorama of data and a new awareness of the premises of his profession.

The Historian and His Sources

Quantitative history presupposes the existence and elaboration of long series of homogeneous and comparable data, and the first problem which presents itself in new terms is that of sources. In general, European archives were formed and classified in the nineteenth century, in accordance with procedures and criteria reflecting the ideological and methodological preoccupations of the period. This meant, on the one hand, that national values predominated and that priority was given to politico-administrative sources. It also meant that documents were preserved and classified in accordance with the special and limited purpose of a particular inquiry: archives were built up to witness to events rather than to time. They were constituted and criticized in themselves and not as factors in a series; the point of reference was external. What was in question was the historical "fact" of the positivists, the naïve mind's illusory sheet-anchor in what is supposed to be real, as distinct from mere testimony—a particular, discontinuous, elusive sequence within either an indefinite flux or a chronology preestablished in terms of centuries, reigns, and ministers. In short,

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archives are the memoirs of nations, just as the letters a person keeps show what an individual has chosen to remember.

But the data of quantitative history refer not to some external, vaguely outlined "fact" but to internal criteria of consistency. A fact is no longer an event selected because it marks a high spot in a history whose meaning has been predetermined, but a phenomenon chosen and sometimes constructed by reason of the recurrence which makes it comparable with others in terms of some unit of time. The whole conception of history based on archives is radically transformed at the very time when its technical possibilities are multiplied by the electronic processing of information. This simultaneous and interconnected revolution in methodology and technique enables us to think in terms of a new kind of archive preserved on perforated tapes. Such archives would not only be built up according to a deliberately planned system; their criteria would also be quite different from those of the nineteenth century. Documents and data exist no longer for themselves but in relation to the series which in each case precedes or follows: it is their comparative value which becomes objective instead of their relation to some elusive "real" substance. Thus, incidentally, the old problem of the *critique* of historical documents moves on to different ground. "External" criticism is no longer based on credibility as derived from contemporaneous texts of another kind, but on consistency with a text of the same kind occurring elsewhere in the temporal series. "Internal" criticism is simplified inasmuch as many of the necessary cleaning-up operations can be entrusted to the memory of the computer.

Consistency is introduced at the outset, when the data are first sorted out, by a minimal formalization of each document which makes it possible to retrieve, over a long period and for each unit of time, the same data in the same logical order. From this point of view the historian's use of computers is not only an enormous practical advance in the time it saves (especially when the sorting is done verbally by tape-recording, as in the Couturier method[3]); it is also a very useful theoretical discipline, in that the formalization of a documentary series which is to be programmed forces the historian from the very beginning to abandon epistemological naïveté, to construct the actual object of his research, to scrutinize his hypotheses, and to make the transition from implicit to explicit. The second critical process, this time an internal one, consists in testing the consistency of the data themselves in relation to those which

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come before and after—in other words, in eliminating errors. It thus emerges as a sort of consequence of the first process, and can in fact be done largely by automation through programmed methods of verification.

Naturally enough, serial history in its manual form began by using those historical series which were easiest to handle, that is, economic, fiscal, and demographic documentation. The revolution introduced by the computer into the collection and processing of data has steadily multiplied the extent to which such numerical series can be explored. The technique can now be applied to any kind of historical data reducible to a language that can be programmed—not only fiscal and legal rolls, but also series of relatively homogeneous literary collections such as medieval chartularies or the cahiers of the states-general of monarchical France.

Thus emerges the first task of serial history, the imperative of its development: the constitution of its subject matter. Classical historiography was constructed from archives worked on and processed according to the critical rules bequeathed to us by the Benedictines of the age of Enlightenment and the German historians of the nineteenth century. The serial history of today has to reconstruct its archives in terms of the dual methodological and technical revolution which has transformed the rules and procedures of history.

This being so, the question arises of the problematical nature of history's subject matter, the hazards of its survival, its partial destruction and sometimes total disappearance. I am not sure this question distinguishes history as much as is sometimes alleged from the other human sciences whose objects are more specifically defined. The characteristic feature of history is the extraordinary and almost unlimited elasticity of its sources. As the researcher's curiosity roves further and further, huge dormant areas of documentation are revealed. What nineteenth-century historian bothered with the parish registers which have now become, especially in England and France, one of the surest sources of our knowledge of preindustrial society?

Moreover, if the researcher invests them with a new significance, sources already exploited once can be used again for other purposes. Descriptions of price movements can lead to sociological or political analyses; Georges d'Avenel is followed by Ernest Labrousse. Demographic series studied from the point of view of, for example, the use of contraception by married couples can also throw light on

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problems of mental attitude or religious practice. Signatures to legal documents can give statistics about the spread of literacy. Biographies systematically grouped in terms of common criteria, on the basis of a given working hypothesis, can build up documentary series imparting an entirely new life to one of the oldest kinds of historical narrative.

Hitherto history has been almost exclusively based on the written traces of men's existence. No doubt live interrogation, which provides empirical sociology with so much of its data, will always be beyond the reach of the historian except for the period in which he lives. But how much unwritten evidence there is still to be catalogued and systematically described. The physical conditions of rural life, the divisions of the land, iconography sacred and profane, the lay-out of early towns, what the houses were like inside—one could go on forever listing the elements of civilization which once catalogued and classified in detail would make it possible to establish new chronological series, and put at the historian's disposal the new subject matter which the conceptual enlargement of history demands. For it is not the sources which determine the approach, but the approach which determines the sources.

Of course, this type of argument must not be pressed too far. To the documentary demands of certain contemporary social sciences history can only answer with irreparable gaps. It is difficult to see what substitutions or extrapolations could ever fill in the columns of an input-output table for the French economy in the time of Henry IV, not to mention periods even more distant. But all this means really is that, conceptually, history is not reducible to political economy. The problem of sources, for the historian, lies not so much in absolute lacunae as in series which are incomplete, and this not only because of the difficulties of inter- and extrapolation, but because of the chronological illusions they may lead to.

Take the classic example of popular revolts in France at the beginning of the seventeenth century. Because of the great abundance of administrative documents relating to the subject at that time, this period has become the best-known chronological sector in the history of peasant risings between the end of the Middle Ages and the French Revolution. The hazards of survival have even seen to it that a large part of these archives, the *fonds Séguier*, ended up in Leningrad, enabling Soviet historians to advance a Marxist interpretation of France's Ancien Regime. The subsequent controversy has enhanced the interest of the documents still further. But another

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problem arises before that of interpretation, and this concerns the presupposition common to both interpretations here: that is, that there really was, during the period when the absolutist state was coming into being and there was probably a rapid increase in taxation, a special chronological concentration of that classic phenomenon in French history, the "jacquerie." The existence of such a concentration could only be definitely established by the study of a long homogeneous series and comparison between this section of it and those before and after. But for several reasons such a series cannot be constructed; in the first place because there is no unique and homogeneous source for such revolts over a long period. Moreover, there is every reason to believe that the survival of such a collection as the *fonds Séguier* in Leningrad, a collection especially rich in this respect, but limited to the papers of one family and thus subject to the hazards and possible distortions of individuals' careers, falsifies our chronological perception of the subject. In any case, a jacquerie is a story without direct sources, a rising of illiterates. We can glimpse it today through the medium of administrative or legal archives, but by this very fact, as Charles Tilly has remarked, every revolt which escapes repression escapes history. The relative richness of our sources during a given period may be a sign of changes which are institutional (reinforcement of the apparatus of repression) or purely individual (special vigilance on the part of a particular official), rather than of any unusual frequency in the phenomenon itself. The difference between the number of peasant risings under Henry II and under Louis XIII may reflect first and foremost the progress of monarchical centralization.

Therefore, in handling serial sources the historian is forced to think carefully about the influence that the way they were constituted may have on their quantitative application. I think we may distinguish between such sources as follows, in order of increasing complexity in their conversion into series.

(1) Structurally numerical sources, grouped together as such and used by the historian to answer questions directly connected with their original field of investigation: for example, French parish registers for the demographic historian; prefectoral inquiries into industrial or agricultural statistics in the nineteenth century for the economic historian; the data on American presidential elections for the specialist in sociopolitical history. These sources sometimes need standardizing (as when there is a variation in local units or a modification of the classifying criterion); also, when there are gaps in the

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documentary sequence, one may have to extrapolate certain elements. But in such cases both operations are carried out with the minimum of uncertainty.

(2) Sources which are structurally numerical but used by the historian substitutively, to find the answers to questions completely outside their original field of investigation. For example, the analysis of sexual behavior on the basis of parish registers, the study of economic growth through price series, the socioprofessional evolution of a population through a series relating to taxes. Here the historian encounters a double difficulty. He has to define his questions all the more meticulously because the documentary material was not assembled with them in mind, and the question is constantly before him of the relevance of such material to such questions. He usually has to reorganize the material completely in order to make it usable, and in so doing makes it more arbitrary and so more open to objection.

(3) Sources which are not structually numerical but which the historian wants to use quantitatively, by a process involving two substitutions. In such cases he has to find in his sources a univocal significance in relation to the question he is asking. He also has to be able to reorganize them in series, that is, in comparable chronological units, and this demands an even more complex process of standardization than in (2). Data of this third type, which become more and more frequent the farther one goes back into the past, can be subdivided into two classes. First, nonnumerical sources which are nevertheless serial and thus easily quantifiable, as for example modern European marriage contracts drawn up by notaries, which, according to the historian's choice, can give evidence about endogamy, social mobility, income, literacy, and so on. Secondly there are the sources which are strictly qualitative and therefore not serial, or at least particularly difficult to standardize and arrange in series, as for example the administrative and legal series referred to above, or iconographical survivals of forgotten faiths.

But whichever kind of source he is dealing with, the historian of today has to rid himself of any methodological naïveté and devote a good deal of thought to the way in which his knowledge is to be established. The computer gives him the leisure to do so by freeing him from what used to take up most of his time—the recording and card-indexing of data. But at the same time it demands from him rigorous preliminary work on the organization of series and their meaning in relation to the inquiry. Like all the social sciences, but

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perhaps with a slight time-lag, history is passing from the implicit to the explicit. The encoding of data presupposes their definition; their definition implies a certain number of choices and hypotheses, made all the more consciously because they have to conform to the logic of a program. And so the mask finally falls away of that historical objectivity which was supposed to lie concealed in the facts and to reveal itself at the same time as them. Henceforward the historian is bound to be aware that he has constructed his own facts, and that the objectivity of his research resides in the use of correct methods for elaborating and processing them, and in their relevance to his hypotheses.

So serial history is not only, or even primarily, a transformation of the raw material of history. It is a revolution in the historiographical consciousness.

The Historian and His Facts

The historian, working systematically on chronological series of homogeneous data, is really transforming the specific object of his knowledge—time, or rather his conception and representation of it.

(1) The so-called *histoire événementielle* is not to be defined by the preponderance it gives to political facts. Nor is it made up of a mere narrative of certain selected "events" along the time axis. First and foremost it is based on the idea that these events are unique and cannot be set out statistically, and that the unique is the material par excellence of history. That is why this kind of history paradoxically deals at one and the same time in the short term and in a finalistic ideology. Since the event, a sudden irruption of the unique and the new into the concatenation of time, cannot be compared to any antecedent, the only way of integrating it into history is to give it a teleological meaning. And as history, especially since the nineteenth century, has developed primarily as a mode of interiorizing and conceptualizing the sense of progress, the "event" usually marks some stage in the advent of a political or philosophical ideal—republic, liberty, democracy, reason, and so forth. The historian's ideological consciousness can assume very subtle forms. It may group knowledge relating to a certain period around unifying schemas not directly linked to political options or values: for example the spirit of an age, its *Weltanschauung*. But basically the same compensating mechanism is at work: in order to be intelligible the event needs a general history apart from itself and independently determined.

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Hence the classic conception of historical time as a series of discontinuities described in the mode of continuity—that is, as narrative.

Serial history, on the other hand, describes continuities in the mode of discontinuity: it is a problem-history instead of a narrative one. Because it has to distinguish between the levels of historical reality, it breaks down all previous conceptions of general history, calling in question the old postulate that all the elements of a society follow a homogeneous and identical evolution. The analysis of series only has meaning if it is done on a long-term basis, so as to show short or periodic variations within trends. The series reveals a time which is no longer the mysterious occasional spurt of the event, but an evolutionary rhythm which is measurable, comparable, and doubly differential in that it can be examined within one series or as between two or more.

A wedge has thus been driven into the old carefully enclosed empire of classical historiography, and this by means of two distinct but connected operations. First, by the analytical breakdown of reality into different levels of description, serial history has opened history in general to concepts and methods imported from the more specifically constituted social sciences such as political economy, which has probably been the operative factor in the recent historical revival. Secondly, by quantitatively analyzing the different evolutionary rhythms of the different levels of reality, it has at last turned into a scientifically measurable object the dimension of human activity which is history's raison d'être—time.

(2) Now that the historian's hypothesis has shifted from the level of the philosophy of history to that of a series of data both particular and homogeneous, it usually reaps the advantage of becoming explicit and formulable. But at the same time historical reality is broken down into fragments so distinct that history's classic claim to give a universal view of things is endangered. Must the claim be abandoned?

I would say it may probably be kept as a goal on the horizon, but that if history wants to go forward it should abandon this ambition as a point of departure. Otherwise it might fall once more into the teleological illusion described above. Present-day historiography can only progress insofar as it delimits its object, defines its hypotheses, and constitutes and describes its sources as carefully as possible. This does not mean it has to restrict itself to microscopic analysis of one chronological series. It can group several series together and put forward an interpretation of a system or subsystem. But today a

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comprehensive analysis of the "system of systems" is probably beyond its power.

We may take as examples demographic and economic history, the most advanced sectors now in France and probably elsewhere. It so happens that for the past twenty years or so the period which in France is called "modern"—that is, the period between the end of the Middle Ages and the beginning of the nineteenth century—has been the subject of the largest number of studies in serial history, both demographic and economic. So it is the one we are least ignorant about from this point of view. French historiography,[4] starting out from legal records and reconstructed prices, has compared these with the evolution in the number of people as shown in demographic series. Thus there has gradually been built up the concept of an "economic Ancien Regime," based on the preponderance of a cereal production exposed to the vagaries of meteorology, and on the periodical purging of the system by recurrent crises. These crises are indicated by sudden steep rises in price curves and the collapse of those indicating size of population.

But the price series, the meanings of which can be quite varied and ambiguous, have been supplemented by more specific indicators concerning volume of production, and by the use of series suggesting the evolution of supply and demand, itself a factor in the evolution of prices. On the subject of production, though the tithe records concern the same percentage of the harvest every year and so tell us nothing absolute, they are valuable because of their relative comparability. For production we also have the proto-statistical sources brought together by the administration of the Ancien Regime and possibly reorganized on a national scale. On the subject of demand, in addition to general demographic records, we can also turn to the reconstruction of the great masses of liquid money: the treasuries of communes and seigneuries, tithes, rents, profits, wages.

The combination of many demographic and economic series has recently enabled Professor E. Le Roy Ladurie to make a wider analysis of the old agrarian economy.[5] His book gives a sampling of data covering the whole of the Languedoc, a long-term chronology (from the fifteenth to the eighteenth century), and a rich and varied quantitative documentation. Thanks to the cadastral surveys, the latter makes possible a study of rural property-owning. The fifteenth to the eighteenth centuries are the story of a very long agrarian cycle characterized at once by a general equilibrium and by a series of states of disequilibrium. The general equilibrium roughly corre-

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sponds to the Malthusian model—the model which Malthus discovered and made immortal at the very moment it ceased to be true, at the time of England's take-off. The economy of early rural Languedoc is dominated in the long term by the relation between agricultural production and the number of men. Society's inability to raise agrarian productivity and the absence of an unlimited reserve of cultivable land, together with the famous "monetary famine" beloved of the price historians, all presented structural obstacles to decisive growth. Though the monetary explanation loses its central role, it is integrated into a multiple and unified interpretation.

The structure of the old economy acted in the long term as an internal regulator. Nevertheless, within the system, the different variables—number of men, evolution of property, distribution of income from rents, fluctuations in productivity and prices, and so on—make it possible to distinguish separate periods in accordance with the position each variable occupies in relation to the whole, in terms of the annual rhythms and cycles of each particular curve. The complete structure thus chronologically comprises several types of combinations of series, that is, several different situations. And in fact it is through the careful examination of these successive situations, and the features they have in common or in which they differ, that the structure itself emerges. This, incidentally, may shed some light on the dispute over synchronic and diachronic which often divides anthropologists and historians, and which at present is at the heart of the evolution of the social sciences. The short- or medium-term periodic movement which constitutes an "event" on the economic plane does not necessarily clash with the theory of general equilibrium. On the contrary, an empirical description of such movements may make it possible to define the theoretical conditions of the equilibrium, the elasticity of which indicates the limits within which it operates.

(3) But the Languedoc example quoted above is a special case in that the correlation there between the different demographic and economic series is made within a comparatively homogeneous region and a limited sphere of human activity (agrarian economy). The sectional application of serial history to different areas usually leads to the analysis of regional or national disequilibria. And general, or would-be general, serial history, even when restricted to a limited geographical area, tends to lead to the analysis of disequilibria in time between the different evolutionary rhythms of different levels of human activity.

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(3a) The first point is now well known, thanks to the increasing number of studies on regional economic history. The specialist is used to the idea of there being measurable differences between different countries, and between areas influenced in different degrees by the same situation or reacting in different ways to similar situations occurring at different times. There are countless examples, some of which raise problems which have become classic in European history. Such, for instance, are the recently revived question of comparative growth as between France and England in the eighteenth century;[6] the antithesis between the rise of agriculture in Catalonia in the eighteenth century and its decline in Castile, which has been shown by P. Vilar;[7] or the contrast in seventeenth-century France between the Beauvaisis revealed by Goubert,[8] poor, and seriously stricken by the middle of the century by economic and demographic recession, and the Provence described by Baehrel,[9] comparatively fortunate, or at least not affected by the turndown of expansion until appreciably later. More generally, the date of this reversal, this plunge into the "tragedy" of the seventeenth century, varies considerably according to region and to the nature of the local economy. It becomes increasingly unlikely that there is only one economic *conjoncture* obtained for both urban and rural economies.[10]

Thus serial history opens out at once into the analysis of situations either differential or simply separated from one another in time (in other words, into what might be called the geography of serial history's chronology), and into the study of structural differences which may indicate chronological discrepancies. Cycles occurring at different times in the same or different regions, but fundamentally comparable internally, only exhibit geographical variations of the same theme. Contradictory developments, on the other hand, whether within the same geographical area (for example, as between town and country), or between two different areas, may present the historian with differences in economic structure.

(3b) But history cannot be reduced just to the description and interpretation of economic activity. If it has a specific character distinguishing it from the other social sciences, this consists precisely in having no specific character, and in claiming the right to explore time in all its dimensions. It is easy to see why economics has been the primary sphere of quantitative history—because of the necessarily measurable character of its indicators; by the preciseness of the concepts it makes available; and by its theoretical approach in

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terms of growth, the favorite image for historical change in Western thought today. But man is not merely an economic agent. The contemporary world offers too many examples of cultural resistance to the general adoption of growth on the Western model for the historian not to mistrust the Manchester school approach to progress (or its Marxist inversion). He is bound to want to analyze the societies of the past in terms of politics and ideology as well as economics.

But even so he does not and cannot revert to the old teleological history of progress, which extrapolates into cultural life the rhythms of economic development, whether this is supposed to occur by a kind of peaceful, natural adaptation or through the necessary medium of revolution. These ideological postulates of another age are useless now; it is not by clinging to them that the historian can preserve the universality of history. The only way to do so is by setting out to list and describe by the methods of serial history the other levels of human activity besides the objective processes of economics; by starting from the hypothesis that chronological rhythms, the researcher's attitude to time, may vary according to the different levels of reality or to the particular part of the system being analyzed.

On the practical plane almost everything still remains to be done. The historian must examine what may be the indicators, quantifiable or otherwise, of what may be called a "politico-ideological" society; he must establish its documentation and what constitutes representativity and comparability from one period to another. For all this there are sources as abundant and series as homogeneous as in the field of economics or demography: they exist for popular literacy;[11] the sociology of education and religious sentiment; the absorption of ideas by elites; the manifest or latent contents of political ideologies; and so on. On the theoretical plane the main thing of course is to build up gradually the components of a comprehensive history, but first and above all to analyze the different rhythms of development at various levels of a historical complex. This is the only way to achieve two of the priorities of historiography today. These are:

(1) To revise the traditional general periodizations, which are mainly an ideological inheritance from the nineteenth century, and which presuppose precisely what is still to be demonstrated, that is, the roughly concomitant development of the most diverse components of a historical complex within a given period. Instead of

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beginning from a set of periodizations, it is probably more useful to start by examining the components concerned. It is probable, for example, that while the concept of the Renaissance is relevant to many of the indicators of cultural history, it is devoid of meaning in relation to the data for agricultural productivity.

(2) The problem then is to define, within a complex of data of different kinds, which levels are developing rapidly or changing decisively, and which in the medium or long term are in a state of inertia. It is not clear, for example, that the dynamism of French history from, say, the great expansion of the eleventh–twelfth centuries is economic in character: it may be that educational, cultural (in the broad sense), and state investment (the latter through the various public offices), play a more fundamental role here than increase in the national product. Perhaps I shall be allowed to conclude on this bold hypothesis if I add that it will remain unverifiable until general history has sat at the feet of serial history.

This article was translated from the French by Barbara Bray.

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M. I. FINLEY

Archaeology and History

MOMIGLIANO BEGAN his elegant obituary of Mikhail Rostovtzeff by recalling the "unforgettable impression" made on students of his (and my) generation by the publication in 1926 of the *Social and Economic History of the Roman Empire:*

> All seemed, and indeed was, extraordinary in the book. Even the external appearance was unusual. We were accustomed to books on ancient history where the archaeological evidence, if used at all, was never presented and explained to the reader. Here a lavish series of plates introduced us directly to the archaeological evidence; and the caption of each plate really made us understand what one could learn from apparently insignificant items . . . Rostovtzeff delighted and surprised us by what seemed to us his uncanny gift of calling ancient things to life. He guided us through the streets of Rome, Pompeii, Nîmes and Trèves and showed how the ancients had lived.[1]

Half a century later the situation has radically altered in some respects. Although there are still too many books written about ancient history in which the archaeological evidence receives no more than lip service, not merely in the presentation but also in the historian's own study, it is also true that classical and Near Eastern historians, as a whole, are much more conscious of, and knowledgeable about, archaeological evidence than they were when Rostovtzeff's book appeared.[2] If it is hard to think of anyone since who possesses his gift of the illuminating caption, that is sad but not very significant. What is serious is that the demands made by historians of antiquity upon archaeology, and the methods of employing and presenting archaeological evidence in ancient history, have not advanced much beyond what was possible for Rostovtzeff, and often lag behind. Yet in the same half-century archaeology itself, at its best, has advanced immeasurably, and historians ought to have advanced with it.

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However, it is not my intention in this essay to produce a catalogue of grievances along these lines; it would soon become boring and unilluminating. Looking to the future, there are two central questions, in my judgment. The first is whether current trends in archaeology are departing so far from the kinds of questions historians have traditionally put to archaeologists that the gap between the two will soon be widened rather than narrowed. That will be my concern in the first section; to my mind, at least, there is a close kinship with the problems created by the trend toward "serial history" (by whatever name it is called) dealt with elsewhere in the present volume. Then I shall go to my second question, limited to classical archaeology alone: What is it that the classical historian ought to be asking of archaeological evidence today, and how successfully are the archaeologists, from their side, adjusting their own older aims and techniques to these new demands? Implicit in this discussion is the assumption that contemporary historians, even of antiquity, *are* asking new kinds of questions. Anyone who is happy with kings and battles or with "calling ancient things to life" (I deliberately resort to a caricature) will find the discussion wholly irrelevant.

I

In common with other disciplines in the humanities and social studies, archaeology gives the appearance of being in a crisis. That is attested by the spate of books and articles with such titles as *New Perspectives in Archaeology*.[3] At the simplest level, there is a strong reaction within the discipline against the familiar excursions into prehistoric religion, economics, or art appreciation that are neither grounded in, nor controlled by, theory or adequate knowledge. There is a new mood of austerity, even of pessimism. It may seem "hard doctrine to some people," writes Stuart Piggott, but "the observational data of prehistory seem to me in almost every way to be more ambiguous, and more capable of varied interpretations, than the normal run of material available to historians. What we have at our disposal, as prehistorians, is the accidentally surviving durable remnants of material culture, which we interpret as best we may, and inevitably the peculiar quality of this evidence dictates the sort of information we can obtain from it. Furthermore, we interpret the evidence in terms of our own intellectual make-up, conditioned as it is by the period and culture within which we were

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brought up, our social and cultural background, our current assumptions and presuppositions, and our age and status."[4]

One example demonstrating the urgent necessity for such "hard doctrine" merits examination—the remarkable fable of the Great Mother Goddess. For Jacquetta Hawkes that goddess was so omnipresent and omnipotent in Bronze Age (Minoan) Crete that the civilization itself is labeled a "predominantly feminine force." The case rests on a group of small Neolithic figurines, averaging less than two inches in height, which she describes as follows: "The material evidences of the religious life of these Stone Age farmers consist very largely of formalized statuettes of women (carved or modelled) which they kept in their homes or sometimes provided with sacred houses of their own. In making these images they called attention to the reproductive function, giving them huge breasts and buttocks, and often the mountainous bellies of advanced pregnancy. Moreover they usually set them in a squatting position, which at that time may have been the accepted position for childbirth."[5] Apart from Miss Hawkes's private fancy about feminine and masculine forces in civilizations, her interpretation of the figurines is a restatement of what had become virtually received doctrine in archaeology.[6] It has now been shattered beyond hope of rescue by the publication of Peter Ucko's book on the figurines.[7]

The total number of Neolithic Cretan anthropomorphic figurines known by 1969 was 103. Of these, only 28 are certainly female, 5 male, and 28 sexless; the remainder cannot be classified, primarily because of their fragmentary state. They sit, stand, kneel, and crouch as well as squat. "The majority of female figures have close-set breasts which are flat and small and which project very little from the body," and anyway the assessment "is almost always largely subjective." Only two figurines have come from houses anywhere in Crete, none from any structure which can be identified as a shrine. Usually the find site can be described only as "general habitation debris." And there are also numerous animal figurines.

The received view is thus an extreme case of the type that D. L. Clarke, a leading Young Turk in the field, criticized when he wrote, "The degree of confidence that we are logically justified in placing in many archaeological generalizations is often undermined by failure to specify the proportion of observed cases, the variety of circumstances or the existence of conflicting examples."[8] The wonder is that no one before Ucko had bothered to examine systematically all the 103 Cretan figurines.[9] Now "archaeologists and students of

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prehistoric religion" are asked to "overhaul present theories very critically."[10] Beyond doubt they must, but it is not mere *Besserwisserei* to point out that some of us—archaeologists as well as historians and anthropologists—have been challenging the Mother Goddess for years. Even without Ucko's breakdown, other methodological objections had been raised—for instance, that the Mother Goddess devotees made no effort to explain the complete disappearance of these figurines in the Minoan period, and offered no foundation for their vast superstructure other than the vaguest subjective verbiage about the "meaning" of big breasts and heavy buttocks. It should be recorded that Ucko had published his Cretan analysis as early as 1962 in the authoritative official organ of the Royal Anthropological Institute,[11] with little visible impact.

How, then, do we interpret such remains as the anthropomorphic figurines? The archaeological Young Turks reject Piggott's kind of pessimism. "The argument," writes L. R. Binford, "that archaeologists must limit their knowledge to features of material culture is open to serious question; and second, the dichotomy between material and nonmaterial aspects of culture itself and the relevance of this dichotomy for a proposed hierarchy of reliability have also been the subject of critical discussion . . . It is virtually impossible to imagine that any given cultural item functioned in a socio-cultural system independent of the operation of 'non-material' variables. Every item has its history within a socio-cultural system—its phases of procurement of raw material, manufacture, use, and final discarding . . . *There is every reason to expect* that the empirical properties of artifacts and their arrangement in the archaeological record will exhibit attributes which can inform on different phases of the artifact's life history" (my italics).[12]

Of course no one imagines that cultural items function independently, least of all the pessimists whom Binford is attacking. The issue lies in the final sentence I have quoted. Is there any reason to expect what Binford expects, and significantly can offer only as an expectation rather than as a proposition for which there is available evidence? On the contrary, there is sufficient evidence that identical artifacts and arrangements of artifacts can result from different socioeconomic arrangements of procurement, manufacture, or distribution. For example, we know from the chance preservation of accounts inscribed on stone, that the most delicate stone carving on the temple in Athens known as the Erechtheum was produced by free men and slaves working side by side at the end of the fifth

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century B.C. Nothing in the material remains (the carving itself) could have told us that. On the other hand, the surviving accounts of the temple of Apollo at Epidaurus, built thirty or forty years later, are of such a nature that the labor force is not specified. How does Binford imagine it will be possible to discover whether or not slaves were employed, at the highest skill level, on that temple?[13]

The most radical, elaborate, and sometimes brilliant argumentation will be found in Clarke's *Analytical Archaeology* already mentioned. For Clarke the crisis in archaeology is most obviously exposed by the attempt to produce "imitation history books."[14] His objective is to establish archaeology "as a discipline in its own right, concerned with archaeological data which it clusters in archaeological entities displaying certain archaeological processes and studied in terms of archaeological aims, concepts and procedures . . . The entities, processes, aims, procedures and concepts of archaeology have a validity of their own in reference to the archaeological frame and despite their generation by—and partial correlation with—former social and historic entities."[15] The "fundamental" archaeological entities are "the attribute, artefact, the artefact-type, the assemblage, the culture and the culture group."[16] The major aims of the discipline are (1) "the definition of the fundamental entities"; (2) "the search for repeated similarities or regularities in form, function, association, or developmental sequence amongst the particular entities from every area, period and environment"; (3) "the development of higher category knowledge or principles that synthesize and correlate the material at hand whilst possessing a high predictive value. The development of increasingly comprehensive and informative general models and hypotheses."[17]

We know where we are now; the familiar polemic of the social scientist against history has been carried back into prehistory, though I am unable to discover in his more than six hundred pages what he means by "a general archaeological theory." Insofar as Clarke rests on a tautology—"an archaeological culture is not a racial group, nor a historic tribe, nor a linguistic unit, it is simply an archaeological culture"[18]—his sophisticated statistical procedures, requiring extensive use of computers, will no doubt enhance and refine archaeological inquiry considerably. However, it is apparent that he does not wish to be taken too literally. That is revealed by the way the word "function" creeps into the second of the "aims," and by the conventional admission of social anthropology into the exclusive club.[19] When he speaks of "religious data" and "religious artefacts," and

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writes a short paragraph about Minoan Crete that includes such phrases as "embalmed within the conservative memory of the religious subsystem," "the later Minoan idealized bull cult," "a religious 'memory' or a cultural 'dream' of such a former trajectory,"[20] one is entitled to protest not only that he has abandoned "archaeological procedures" but that he has descended to the excesses of the Mother Goddess fable.[21]

Furthermore, when Clarke leaves prehistory for periods for which there is some documentation, other than archaeological, he is surprisingly happy if his archaeological analysis seems occasionally to coincide with written documentary evidence. I say "seems to" because it does not always do so as he claims, and because when it does, the coincidence is illusory. Of the former failing, I merely cite the inaccuracy and the misunderstanding of the nature of such Greek sources as Homer, Herodotus, and Eratosthenes, which pervade the paragraph that is supposed to provide literary substantiation of his account of Hallstatt assemblages from the ninth to the fifth centuries B.C.[22] For the second, there is a more significant example. Clarke reports a computerized analysis of more than one thousand "burial assemblages of weaponry and harness artefacts" of Turkic-speaking nomads between about A.D. 900 and 1300. The analysis produced a diachronic classification into four main groups, the transition to the fourth being marked by the replacement of horse gear in the women's graves by "distinctive new Mongolian types . . . earrings, mirrors, birch-bark head-dresses." "Historically," Clarke adds, "and by coin evidence it was possible to associate this change with the period of the Golden Horde invasions."[23] Though the observations are no doubt accurate, the obvious comment is that, granting the Mongolian character of the objects can be established from the objects themselves, no "archaeological procedures" would have revealed the "Golden Horde invasions," as Clarke's own wording concedes. Why, then, does he resort to what he has earlier labeled, pejoratively, a "nostalgic retreat into historiography"? Why does he not leave the final transition as "unhistoriographical" as the other three, for which there appear to be no distracting coins or documents?

These questions raise the fundamental one of the purpose of the exercise. Why expend effort and financial resources to sort out in a time-series some four hundred years of burial assemblages—or to sort out, in any fashion, any group of past phenomena, prehistoric or historic? Is it only in order to achieve a more or less abstract classifi-

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cation of objects? To ask the question is to answer it. No one can make a claim for such a bloodless procedure, beyond the aesthetic one that can equally be made of a game of chess, the claim that elegant precision has value for its own sake. Unless one is prepared, in the study of the past, to abandon all interest in change, growth, evolution, in institutions, events, or the interrelationships among the different aspects of human society, I see no virtue in the insistence that any one type of evidence, or one type of analytical procedure, must be isolated from all other types—no virtue, in short, in Clarke's "validity of their own despite their correlation with social and historical entities." No more do I see any virtue, as I have already said, in the practice of historians who similarly neglect archaeological materials. And perhaps I should repeat that, on all available evidence, it is impossible to infer social arrangements or institutions, attitudes or beliefs from material objects alone.

None of this is an argument against more and better quantification of evidence. But it is an argument against the view, sometimes expressed explicitly, that what cannot be quantified cannot be dealt with at all, that only quantification produces "scientific" (as distinct from "subjective" or "ideological") analysis and results. The objection is simply that large areas of human behavior thereby disappear from the "scientist's" view altogether, including all historical events or series of events. This is the point at which I believe the Young Turks in archaeology and in history meet. Historical demographers are now able to provide complicated graphs of the trends in life expectancy, size of family, illegitimacy over long periods of time. These are important data for the history of the family, but they are not equal to the history of the family, and never will be. Likewise, it is interesting to know that central Asian women took horse gear to the grave with them in one period, and in another preferred earrings and mirrors, but no table of correlations will reveal the thinking and the value judgments behind the habits.[24] Admittedly the latter kind of interest requires speculation, with a low coefficient of reliability, and explanations cannot be verified in the same way as frequency tables can be, but that seems insufficient ground for dismissing serious, disciplined efforts as mere "counterfeit history."

II

It is self-evident that the potential contribution of archaeology

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to history is, in a rough way, inversely proportionate to the quantity and quality of the available written sources. It is also self-evident that the line between prehistory and history is not a sharp one, that for centuries after the introduction of writing, the historian's evidence remains almost exclusively archaeological, at least for some civilizations, notably the Greek and the Roman. Perhaps the most frustrating example is that of the Etruscans: despite some 10,000 more or less deciphered texts and a considerable, though late and distorted, Roman tradition behind them, assemblages of artifacts remain not only the base of all accounts but nearly the whole of the evidence. An Etruscan tomb is nothing more than an assemblage of artifacts, despite the sophistication of the technology or of the wall-paintings, so long as there is no adequate literary key to the conventions and values represented by the artifacts. Nowhere is Piggott's hard doctrine more necessary, and nowhere is it more systematically ignored in a continuous outpouring of "counterfeit history"—with notable exceptions that need not be named here, though I cannot refrain from noting that what needs to be said in protest against the methods that produce that counterfeit history will be found in Massimo Pallottino's *The Etruscans*.[25]

For the earliest historical periods, an extraordinary complication is introduced by oral tradition and historical legends. The problem is then not simply one of correlating archaeological and literary evidence but of using archaeology to assess whether, and how far, the literature has any worth at all. How difficult that is, and how little agreement has been reached so far, in large part because of unclarity about the canons of discrimination, are clear from the current debates about the Minoan and Mycenaean civilizations and the Trojan War.[26] Apart from the anomalous Linear B tablets, there is no contemporary written evidence for this long period, and it remains the province of the prehistorian rather than of the historian; in the final analysis the work is one of reconstruction from archaeology, even if one is prepared to take more from the legends than I am.

The earliest Roman history presents a different situation, because it began late enough to fall within the period of literacy and because the Roman traditions about their origins, unlike the Greek, have the external appearance of a very detailed history, full of narrative and of constitutional and institutional information ordered in a coherent time sequence. Roman history cannot be said to begin before the period of Etruscan rule, the sixth century B.C., and the contemporary documentation for the following century, the first

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period of independence, though scrappy, includes annalistic fragments and the Twelve Tables.[27] The "histories" are of course very late—the early books of Livy, who died in A.D. 17, and of his contemporary, Dionysius of Halicarnassus, and several lives by Plutarch, still later by about a century—and largely fictitious. However, Theodor Mommsen and others had already demonstrated, long before there was any relevant archaeology of value, that there are techniques by which some data could be pulled out, chiefly in the constitutional, legal, and religious spheres, to which one could with some confidence attach the name of genuine history. Today we have available a full synthesis of the archaeology of early Rome and its environs, unrivaled in Graeco-Roman archaeology in its completeness, and it is important to assess its impact on the received view of early Roman history.[28] In the first place, by itself it reveals perhaps less than some had hoped, in broad terms that sixth and fifth century Rome was a fairly primitive village, with strong Etruscan traits, far removed from the great Livian Rome of the kings and early Republic, but one that was rising in its material level and its size by the end of the fifth century. In the second place, archaeology has confirmed the chronological outlines of the tradition—that is to say, historians are satisfied that it has, whereas the individual archaeologists most directly involved in the discussion insist, for some reason I am unable to fathom, that the literary chronology must be downdated by about half a century.[29] And in the third place, we are in all other respects left with no tools of analysis other than those inherited from the nineteenth century, refined, occasionally supplemented by new epigraphic fragments, checked by modern comparative studies, but unchanged in their fundamental nature.

From this particular, pessimistic report it is tempting to draw the rather paradoxical conclusion that the contribution of archaeology to history becomes greater as the volume and reliability of non-archaeological evidence increase (until the latter reaches the volume and nature of modern, even early modern, documentation). There is truth in that proposition, I believe, but not the whole truth, as I shall illustrate in a moment. It would be pointless to catalogue the many important contributions archaeology has in fact made to our knowledge of ancient history. Instead, I have selected three further examples that raise major problems about future possibilities and needs, examples that fall within the field of economic history.

The first example is at the same time a demonstration of the limits of my paradoxical proposition—the history of what is convention-

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ally called Greek "colonization," in reality the history of Greek expansion between about 1000 and 550 B.C., to Asia Minor and the coastal areas surrounding the Black Sea in the east, to southern Italy, Sicily, and along both shores of the Mediterranean in the west. The Greek tradition, scattered in a multitude of writers from Herodotus to Eusebius, consists of a chronological framework (and by the end in very precise dates), anachronistic propaganda on behalf of the Delphic oracle, and anecdotes. No history of colonization was possible on that basis. Archaeology has confirmed the chronological frame, though of course not the precise dates (as with early Rome), but it has also achieved very much more. In combination with the literary materials, it has made possible a kind of history, not a narrative political account but a picture of settlement, growth, and movement, of urban organization, of trade and manufacture, of relations with the native populations, of cults and temples. The picture exists only in broad outlines, it is very incomplete, much is uncertain and controversial, but hardly any of it could be derived from the ancient traditions alone—or from the archaeology alone.[30] The lines of further archaeological exploration are now clear—and so are the difficulties and weaknesses in our methods of interpretation.[31]

The second example is the history of money and coinage in antiquity. Systematic study began in the eighteenth century, but it was dominated, almost monopolized, by the interests of collectors until the last few decades, and that interest still retains a strong hold. However, the function of coins, as distinct from their rarity or their aesthetics, has become an increasingly prominent subject of research, and the results have been considerable. Discussions of money and coinage are uncommon in both Greek and Roman literature; only from the coin finds can significant conclusions be drawn about the volume of minting, for example, or the circulation of coin. As a preliminary step, it was necessary to devise better techniques for dating Greek coins than the aesthetic canons that once prevailed (since the coins lack dates and, before the Hellenistic period, portraits), and this has been achieved by intensive studies of the dies. It was also necessary to appreciate that coin hoards, not excavation coins, are the fundamental source of material; that hoards must therefore be published promptly and completely; that they must be subjected to proper, and not partial, statistical analysis. Other recent advances need not be enumerated. Nor need it be argued that what we now know, or think we know, about monetary history, as about the history of colonization, rests on archaeological evidence but not on

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"archaeological" analysis. And the interpretation of the archaeological evidence is the point at which the dialogue between historians, numismatists, and archaeologists is still unsatisfactory.[32]

My third example is a curiosity—the perverse refusal by some distinguished experts in the history of technology to permit either the historian or his documentation to make a contribution. They prefer to write the history of technology largely, if not solely, from the artifacts, carrying Clarke's psychology to a *reductio ad absurdum,* without the saving grace of his rigorous techniques. The consequences can be exemplified by comparing the first two volumes of the much praised *A History of Technology,* edited by Charles Singer and others,[33] with the painstaking, often unrewarding, but always more fruitful and more accurate studies during the past decade or so, by scholars who combine the archaeological and documentary evidence in an investigation of agricultural technology in Gaul and other parts of the western Roman Empire.[34]

It will be apparent that much of what I have been calling an advance has been the consequence of a new focus, of new historical interests, and of new questions that the historian had not earlier put to himself, and therefore not to the archaeologists either. What do we now wish from the archaeologist, and, more narrowly, what have the new ways to offer to the ancient historian?

Immediately I must express my belief that the rapid development and application of sophisticated scientific techniques provide only a small part of the answer. Of course they have proved valuable, especially in locating the most promising places to explore. A more interesting example, providing stimulating implications, is the spectroscopic analysis which revealed that the obsidian (volcanic glass) used for tools, by 6000 B.C. at the latest, in a region extending from southern Macedonia to Crete, all came from the island of Melos, where on present evidence there was no settlement for several thousand years after the obsidian mining began.[35] Or, as an example from the historical era, it is not yet possible to determine the source of the marble used in any specific Greek statue or building. The usual, and casual, statements about marble sources in archaeological reports turn out often to have been mere guesses of no value. However, new techniques open up the possibility in the future of answering a complex of questions of the highest interest for social and economic history—questions such as: How much weight was given to costs, especially transport costs, in the decision to select one marble rather than another for any given temple, as against other considerations,

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political, technological (the preference of the architect and the masons), or aesthetic? Until now the information has been so lacking that it has been pointless to tackle such a question (and indeed no adequate study of quarries in antiquity has ever been undertaken). Now the possibility exists, but its realization will require laborious examination on a large scale of widely scattered artifacts and of marble outcroppings.[36]

A note of caution about carbon-14 dating is also necessary here. It is a familiar complaint of prehistorians that classical archaeologists do not make proper use of this most widely known of all the new scientific techniques—an unjustified complaint. In the first place, the accumulated experience with carbon-14 dating has revealed more eccentricities and complexities in the method than had originally been suspected. "The archaeologist," Clarke warns us, "can rarely, if ever, date excavated strata and their assemblages to precise points in time—all archaeological dates are relative, even the so-called absolute isotope decay datings."[37] Furthermore, relative dates, or dates ±50 or 100 years, are virtually useless for the late Bronze Age, let alone the historical age. They will not help resolve such current controversies as the date of the destruction of Cnossus, the connection between the volcanic explosion at Santorini and the destruction of sites in eastern Crete, the dates of the earliest Greek colonies in southern Italy and Sicily.

In the end, what ancient historians now require of archaeologists is something much simpler, much more primitive—a willingness to devote themselves to precisely formulated historical questions and a far greater consciousness of the value of statistics, for which pencil and paper and elementary numeracy are on the whole sufficient, though a simple computer would do no harm. Rostovtzeff made the point directly a generation ago. In discussing the extraordinary position of Hellenistic Rhodes as the greatest entrepot and financial center, and perhaps the wealthiest city, of the Mediterranean world, he wrote:

> We have only slight knowledge regarding the volume and the character of the Rhodian transit trade. I have discussed the evidence which refers to it above and I have mentioned the stamped jar-handles of Rhodes and Cnidus, and the problems connected with them. One of the most important of these problems is that of their respective dates. *A full catalogue of Rhodian stamps will certainly help us to establish their chronology.* Some progress has been made in this direction. We are able to date a considerable group of stamps found at Pergamon (220 to 180). Another group of stamps found at Carthage is certainly earlier than the

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year of the destruction of the city. Thus we have some indications with regard to those stamps which belong to the period between 220 and 146 B.C. . . . *We require comparative statistics* of the various stamps for each place where Rhodian stamps have been found in order to determine whether modern scholars are right in assuming that the Rhodian stamps of the period 220 to 146 are the most common stamps in all the centers of Rhodian commercial activity. *As things stand, we must confine ourselves to some very general statements* [my italics].[38]

That was published in 1941. We still await the full catalogue and the comparative statistics, and we are still confined to very general statements.[39] The reluctance of archaeologists to embark on such projects (comparable to the analysis of marbles I have already mentioned) deserves sympathetic understanding. Archaeology is an expensive activity; patrons are chiefly attracted by the prospect of museum pieces, or of a public sensation. Presumably that is the psychological explanation of the fact that complete catalogues and comparative statistics are becoming increasingly common for coins, geometric pottery, archaic Greek sculpture, and the like, whereas Gjerstad's catalogue of the archaeology of early Rome remains an exception, the catalogue of Rhodian jar-stamps still a desideratum. Yet it is an explanation the historian cannot accept—and he is not by profession competent to do the work himself.[40] Nor is it the whole explanation. A few years ago Snodgrass collected all the identifiable remains of cutting tools and weapons found in mainland Greece (excluding Macedonia) that could be dated in the period ca. 1050-900 B.C. His tabulation of these finds follows:[41]

	Bronze	*Iron*
Swords	1	20+
Spearheads	8	30+
Daggers	2	8
Knives	0	15+
Axe-heads	0	4

No sensation there, yet this simple table answers a question that had not been posed in quite that way before, while vague statements were based on the Homeric poems and guesswork: How rapid was the shift from bronze to iron, not "in general," but in the decisive weapons and implements? Once the chronological question was formulated that way, the only correct way, no alternative remained, if one wished an answer, to the *total* collection and tabulation of the evidence.

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That illustration—and I hasten to add that there are others—explains why I rank an interest in precise historical questions on a par with numeracy and closer attention to statistics in stating what I consider to be the urgent requirement in the immediate future. My own experience suggests that Snodgrass' task was exacerbated by the poor state of the archaeology and the archaeological reporting with respect to graves and grave-goods. I have twice in recent years tried to use such evidence, first for the complicated and ill-understood linguistic situation in Roman Sicily, then for the history of military grave-goods in Greece during the late Bronze Age and the subsequent "Dark Age." Both times I was reduced to impressionistic remarks and to complaints, much like those of Rostovtzeff about the Rhodian jar-stamps. In too few cemeteries have both the excavation and the subsequent publication been systematic and complete.[42] Worse still, it would require a large research project on its own merely to collect the bibliography. Whereas the historian of Anglo-Saxon England has Audrey Meaney's *Gazetter of Early Anglo-Saxon Burial Sites*,[43] I know of no comparable guide to any district of Greece or any province of the Roman Empire.[44]

We have thus come full circle to Ucko and the Mother Goddess. To multiply examples further would be pointless.[45] But it is necessary to look at the other side, at the historian's needs from the viewpoint of the archaeologist.[46] One common objection can be dismissed out of hand: classification and chronology are still uncertain, it is argued, and until they are fixed more firmly, historical questions must wait. That is nothing but Cornford's Principle of Unripe Time. More serious is the plea that archaeologists are already overworked, that there is so much excavation still to be done, that publication would be even further delayed. There is no answer to that, only a choice of values. Of what use, I should reply, is the vast outpouring of annual reports on the year's work from which nothing emerges except the occasional isolated fact, often canceled or corrected in the next year's work or the third or the fourth year's? Of what use are more and more excavations when so many older ones have not been fully reported, and no small number have not been published at all? And, finally, of what use is archaeology anyway, apart from the museum pieces that sometimes come out of the debris, if it leads to nothing more than reports?

One solution that is being mooted to the genuine difficulty created by overwork, insufficient manpower, and insufficient funds is the employment of good sampling techniques. I must demur. No

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sampling technique is "good" except in terms of a predetermined range of questions. It would be a bold archaeologist who believed he could anticipate the questions another archaeologist or a historian might ask a decade or a generation later, as the result of new interests or new results from older researches. Computer experience has produced examples enough of the unfortunate consequences, in just this respect, of insufficient anticipation of the possibilities at the coding stage. A similar failure in archaeology is more harmful, because it is in the nature of archaeological excavation literally to destroy—to destroy forever—whatever is dug up and neither fully recorded nor preserved.[47]

There is of course the crushing retort: *tu quoque.* That I accept, as I have already indicated. Ancient historians are also too often satisfied with impressions, too often nonnumerate and nonquantitative, too often imprecise in the questions they put to themselves, about documentary as about archaeological evidence.[48]

III

The Greek city of Akragas (modern Agrigento) in Sicily was famed for its wealth and its conspicuous consumption in the fifth century B.C. The visible symbol that still survives is the number and quality of its stone temples, perhaps ten of which were constructed in that one century. Ten is an extraordinary number and there are interesting social problems to be studied. But what is a fair standard for temple construction in this period? To answer that question today would involve one in a massive research operation, for the reasons I have already given—the absence of gazetters, and, as I discovered when I looked at the question, of easily available summary statistics even for such major cities as Corinth or Miletus. In the absence of literary or epigraphical documentation, only archaeology can provide the figures (with an admitted margin of error), and the archaeologists have not done so. Temples are not paltry grave-goods; they are central to Greek art history and the volume of publications on the subject grows annually, but always by example alone.

I have introduced this illustration in closing not just to introduce one more complaint, but to pile a second paradox onto my earlier paradoxical half-truth. If it is often the case that the usefulness of archaeology to history increases with an increase in documentation, it is also the case that certain kinds of documentation render

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archaeology more or less unnecessary. If one posed the Akragas question for the Middle Ages, one would find the answer in papal and diocesan records. And one final example. By an ingenious calculation, based wholly on the results of stylistic analysis of remains, R. M. Cook recently arrived at a reasonable estimate of the number of men engaged in the Athenian fine pottery industry in the fifth century B.C.[49] That was an important contribution to economic history. The same question about the English potteries in 1800 would be answered with far greater precision, and with a breakdown by skills and functions which remains out of reach for Athens, simply by going to the pottery archives. It is hardly surprising, therefore, that the relatively new field of industrial archaeology remains a backwater. I should be much more surprised if it ever turns out to be more than that.

References

1. Reprinted in A. Momigliano, *Studies in Historiography* (London: Weidenfeld and Nicolson, 1966), p. 91.
2. Throughout this discussion I mean by "archaeological evidence" only artifacts and I exclude documents—papyri, stone or coin inscriptions, leather scrolls—that excavations produce.
3. L. R. and S. R. Binford, *New Perspectives in Archaeology* (Chicago: Aldine, 1968).
4. Stuart Piggott, *Ancient Europe* (Edinburgh: University Press, 1965), pp. 4-5.
5. Jacquetta Hawkes, *Dawn of the Gods* (London: Chatto and Windus, 1968), p. 6.
6. It is perhaps worth noting that Miss Hawkes is one of the most energetic defenders of the romance of archaeology against hard doctrine and the rapidly increasing employment of a battery of scientific tests; see her article, "The Proper Study of Mankind," *Antiquity*, 42 (1968), 255-262. But she is by no means the only one: see, for example, the regret expressed by R. Wauchope, in his introduction to *They Found the Buried Cities* (Chicago: University of Chicago Press, 1965), that "in their published reports, few archaeologists reveal their hardships and adventures, or even their thoughts and emotions."
7. P. J. Ucko, *Anthropomorphic Figurines of Predynastic Egypt and Neolithic Crete . . .* (London: Szmidla, 1968), part II.
8. D. L. Clarke, *Analytical Archaeology* (London: Methuen, 1968), p. 17.

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9. This point was made by C. Renfrew in opening his review of Ucko's book in *Man,* 4 (1969), 297-298.

10. *Ibid.*

11. "The Interpretation of Prehistoric Anthropomorphic Figurines," *Journal of the Royal Anthropological Institute,* 92 (1962), 38-54.

12. Binford and Binford, *New Perspectives in Archaeology,* pp. 21-22.

13. A comparable argument, based on Bronislaw Malinowski's Trobriand material, will be found in M. A. Smith, "The Limitation of Inference in Archaeology," *Archaeological News Letter,* 6 (1955), 3-7, the most commonly cited statement of what I have been calling the pessimistic position.

14. Clarke, *Analytical Archaeology,* pp. 11, xiii, 3; cf. A. C. Spaulding in Binford and Binford, *New Perspectives in Archaeology,* p. 33: My "argument turns on (1) the abandonment in effect by both groups [archaeologists and social anthropologists] of the notion of historical explanation as a valid category of intellectual activity and, with even more force, of scientific activity."

15. Clarke, *Analytical Archaeology,* p. 13.

16. *Ibid.,* p. 22.

17. *Ibid.,* pp. 21-22.

18. *Ibid.,* p. 13.

19. See B. G. Trigger, "Major Concepts of Archaeology in Historical Perspective," *Man,* 3 (1968), 527-541. This article and the books by Clarke and Binford and Binford provide full bibliographies on the new perspectives.

20. Clarke, *Analytical Archaeology,* pp. 112-113.

21. The Minoan bull-cult desperately needs the same sort of scrutiny Ucko gave to the Mother Goddess.

22. Clarke, *Analytical Archaeology,* p. 392.

23. *Ibid.,* pp. 618-624. The original study is G. A. Fedorov-Davydov, "On Dating Types of Artefacts from Burial Assemblages," *Sovetskaia Arkheologia,* no. 3 (1965), 50-65, reprinted in English in *Soviet Anthropology and Archaeology,* 5, no. 2 (1966), 22-33.

24. Anyone who believes that it may be easy to draw safe inferences from certain types of objects, at least, will profit from reading P. J. Ucko, "Penis Sheaths: A Comparative Study," in *Proceedings of the Royal Anthropological Institute of Great Britain and Ireland for 1969,* pp. 27-67.

25. A new translation, of the 5th edition, is now in press.

26. See, for example, M. I. Finley and others, "The Trojan War," *Journal of Hellenic Studies,* 84 (1964), 1-20.

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27. See E. H. Warmington, ed., *Remains of Old Latin*, III-IV, in the Loeb Classical Library, Harvard University Press.

28. Four volumes of E. Gjerstad's projected six on *Early Rome* have been published so far.

29. See A. Momigliano, *Terzo Contributo alla storia degli studi classici e del mondo antico,* 2 vols. (Rome, 1966), part III, and *Quarto Contributo . . .* (1969), part III; E. Gjerstad, "Discussions Concerning Early Rome," *Historia,* 16 (1967), 257-278.

30. It is fitting that the best general account has been written by an archaeologist: John Boardman, *The Greeks Overseas* (Baltimore: Penguin Books, 1964), a revised edition of which is now in preparation.

31. Attention should be directed to the promising new inquiry now under way, based on the distribution in southern Italy and Sicily of imported Athenian pottery; see the brief report by G. Vallet, with discussion, in *La circolazione della moneta ateniese in Sicilia e in Magna Grecia* (published as a supplement to vols. 12-14 of the *Annali* of the Istituto italiano di numismatica, 1969), pp. 225-237.

32. See my report on classical Greece in *Trade and Politics in the Ancient World,* vol. I of the proceedings of the Second International Conference of Economic History, Aix-en-Provence, 1962 (Paris and The Hague, 1965), pp. 11-35.

33. Charles Singer and others, *A History of Technology* (Oxford: Clarendon Press, 1954-1958). I wrote a harsh review of vol. II in the *Economic History Review,* 2d ser., 12 (1959), 120-125, and I have no reason to retreat in any way. Indeed, my objections are strengthened by the pernicious influence this work continues to exercise; the very recent *Technology in the Ancient World* (London: Allen Lane Penguin, 1970), a popular introduction by an acknowledged expert, H. Hodges, for which the "two main sources" are the Singer volumes and R. J. Forbes's multi-volume *Studies in Ancient Technology* (Leiden: Brill, 1955——), reveals little interest in nonarchaeological evidence, and, it appears from the text, little acquaintance. Forbes, I hasten to add, does not belong to this school of thought.

34. See M. Renard, *Technique et agriculture en pays trévire et rémois* (Brussels: Latomus, 1959); J. Kolendo, "La moissoneuse antique en Gaule romaine," *Annales: économies, sociétés, civilisations* (1960), pp. 1099-1114, and *Postep techniczny a problem sily roboczej w rolnictwie starozytnej Italii* (Polish Academy of Sciences, 1968), with French summary (pp. 184-187 and in *Acta Poloniae Historica,* 18 [1968], 51-62); K. D. White, *Agricultural Implements of the Roman World* (Cambridge, Eng.: University Press, 1967).

35. C. Renfrew and others, "Obsidian in the Aegean," *Annual of the British School at Athens,* 60 (1965), 225-247.

36. See C. Renfrew and J. S. Peacey, "Aegean Marble: A Petrological Study,"

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Annual of the British School at Athens, 63 (1968), 45-66. One instance of how historical conclusions were drawn from a false guess about the provenience of a marble slab on which a decree had been inscribed is fully discussed by A. Georgiadès and W. K. Pritchett, "The Koan Fragment of the Monetary Decree," *Bulletin de correspondence hellénique,* 89 (1965), 400-440.

37. Clarke, *Analytical Archaeology,* p. 45.

38. Mikhail Rostovtzeff, *The Social and Economic History of the Hellenistic World,* 3 vols. (Oxford: Clarendon Press, 1941), pp. 775-776.

39. It is only fair to add that the Rhodian catalogue may at last be in sight, thanks to the sustained efforts of Miss Virginia Grace.

40. See P. Courbin in the introduction to a volume he edited, *Etudes archéologiques* (Paris: S.E.V.P.E.N., 1963), pp. 14-16.

41. A. M. Snodgrass, "Barbarian Europe and Early Iron Age Greece," *Proceedings of the Prehistoric Society,* 31 (1965), 229-240.

42. The situation emerges clearly from M. Andronikos' fascicle, *Totenkult,* in *Archaeologia Homerica* (Göttingen: Vandenhoeck, 1968). Whereas his section on inhumation and cremation (pp. 51-76) permits a number of meaningful diachronic tabulations and formulations (though he has not made them himself, preferring the less revealing technique of continuous prose), the much briefer section on grave-goods (pp. 97-102) hardly advances our knowledge.

43. Audrey Meaney, *Gazetteer of Early Anglo-Saxon Burial Sites* (London: Allen and Unwin, 1964).

44. For the Bronze Age an important start has finally been made by the work of W. A. McDonald and R. Hope Simpson in cataloguing sites in the Peloponnese; see *American Journal of Archaeology,* 68 (1964), 229-245.

45. See the details, chiefly for classical Greece, in S. C. Humphreys, "Archaeology and the Social and Economic History of Classical Greece," *La Parola del Passato,* fasc. 116 (1967), 374-400.

46. These are not hypothetical objections; see *ibid.,* pp. 398-400.

47. See Courbin, *Etudes archéologiques,* p. 15: "Working conditions [in archaeology] have very often destroyed the answers before the questions could be posed . . . Why be surprised, under these conditions, that so many excavations turn out to be virtually useless for anything that is not art history?"

48. See my Aix report, cited in note 32 above.

49. "Die Bedeutung der bemalten Keramik für den griechischen Handel," *Jahrbuch des deutschen archäologischen Instituts,* 74 (1959), 114-123.

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FRANK E. MANUEL

The Use and Abuse of Psychology in History

ALMOST A century ago Friedrich Nietzsche, that history-intoxicated son of the German philological school, delivered a tirade against the hypertrophy of history in the life of his times. In appropriating the title of his essay, I confess to a similar ambivalence respecting the modern uses of psychology in historical studies. After some years of history-writing I have begun to fear that I may be losing my way in the jungles of psychologism. In my predicament I could of course appeal to the analytical philosophers and ask them to enlighten me about the implicit assumptions of my work; but having read their interpretations of the writings of my colleagues I am obdurately resistant. Instead, I shall seek a way out of my perplexity by hearkening to the advice of Alfred North Whitehead, who once said that when a man is lost, he should not ask where *he* is, but where the *others* are. And so, advancing behind a chronological shield, I intend to locate myself by passing in critical review the experience of historians and psychologists whenever they have come into close proximity. After skimming over the eighteenth-century origin of their relationships, I shall concentrate on the last hundred years and even more especially on the recent period, when both disciplines have become mammoth academic enterprises, whose cohabitation, some might say, is doomed to sterility from the outset, like the improbable mating of a whale and an elephant. What my presentation lacks in depth and subtlety, I hope it may achieve in breadth. Perhaps at the end I may find a place for myself, and, who knows, others might be willing to join me even if it means standing midstream in rather shallow waters.

My level of discourse will not be hard-nosed and analytical but descriptive of the goals and achievements of those who sensed the rich potentialities of a new field of expression and proceeded to

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cultivate it. Men have been prospecting for a long time in this region, and I am less interested in the scientific theories they brought with them than in what they have carried back from their expeditions. Though I have not made an actual body count, I suspect that of late there has been more hortatory exposition of what might or should be done than perspective on what has in fact been going on. Commenting on the topos, on the content, of psychological history-writing may prove more illuminating than either dissecting benighted historians who enjoy the *Narrenfreiheit* of blithely going their narrative way, or issuing manifestoes on what would constitute a perfect psychological history.

In the past three centuries major attempts have been made to recover not only the written thoughts of men of other times but their thinking; not only the record of their actions but the secret purposes and hidden, even unconscious, feelings that spurred and accompanied the *res gestae;* not only the literary and artistic objects but the sensibility that was expressed and the emotions aroused in the creators as well as in their contemporary audiences. When there are Pyrrhonists about who question the veracity of recorded history in its most commonsensical usage, how must they regard an undertaking that presumes to re-create inner experiences, which do not usually manifest themselves in clear-cut fashion through specific and forthright documents! And yet this is what psychological history has self-confidently set out to do. Admittedly, this wild intention has always been a minor element in historical narrative since the first Persian chronicles, echoed in the Book of Esther, where we are told what a personage "speaks in his heart." Greek and Roman historians used a variety of devices to disclose the secret purposes and unfulfilled desires animating their protagonists; and the Renaissance historians who imitated them availed themselves of a rich psychological vocabulary in describing the wellsprings of human conduct. It is only since the early eighteenth century, however, that some historians have committed themselves to making the re-creation of inner experience the core of their work, shifting the focus from the deed to the psychic events that transpired in the doer.

I

More and more I see in Giambattista Vico, that lone Italian who lived in Naples from 1668 to 1744, the bold conceptualizer of this

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novel form of historical consciousness. In its exterior aspect his *Scienza Nuova* was a rather conventional theology of history with a cyclical pattern. But if we look beyond the structure, we are amazed to find that he wrote of "tre spezie di natura," three kinds of human nature, and postulated that in each stage of a *ricorso* men had quintessentially different modes of perceiving reality, that not only the physical conditions but the feeling tone of existence was profoundly different in each one, that the very capacity for expression assumed radically different shapes: signs and emblems at one time the only way for mute humans to externalize feelings; poetry the only speech of barbaric men; and not until the rational epoch the voice of reason in prose. Every stage in the cycle was marked by its own balance between rational faculties and aggressive violence, between terror of death and a desire for convenience, between robust imagination and calculated punctiliousness. In sum, the nature of living—of thinking, feeling, and willing, the three traditional faculties—had undergone revolutions in time. And the changing quality of existence was discernible in the history of language, in literature and laws, and in visual arts like painting.

How was it possible for Vico, a man of the early eighteenth century, to interpret that evidence, to recapture the emotions and spirit of the age of heroic barbarism? His answer was that the cycle of history was imbedded in the human soul. Understanding of the transformations was possible because men in fact themselves lived through the whole of the cycle from primitivism to rationality in the course of their development from earliest infancy through adulthood. And all about in the world there remained vestiges of primitive mentality in savage countries and perhaps in the behavior of women.

The great French romantic historian of the next century, Jules Michelet, translated and commented on the works of Vico when they were hardly known in Western culture. Michelet's voluminous history of France can be conceived as an effort to explore the changing consciousness of Frenchmen in their thousand years of national life. His dramatization of the Renaissance as a new way of perceiving the world was an innovation in the uses of psychology in history entirely in the spirit of Vico's "New Science." By mid-nineteenth century Michelet had at his disposal more refined psychological tools, some inherited from the utilitarian tradition of the Enlightenment, others derived from the works of Jean-Jacques,

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that great revealer of the previous age, who had made himself transparent and bared his own complex private world as a model for every man.

In the latter part of the eighteenth century the Germans had a parallel to Vico in Herder, who imagined that through the course of world history an infinite number of human aggregates would be fashioned in isolation by the physical and climatic conditions of their living-space and that each one, in a totally unique way, would forge for itself an idiosyncratic balance of sense perceptions which no other *Volk* could imitate. This *Volk*-genius was early embodied in a mythology, a religion, a poetry, in short a *Volk* culture, within the confines of which men of that *Volk* were forever fixed. In the later efflorescence of a culture, elements of reason might appear, but for all time every work of literature, art, and music was in its essence a reflection of the primitive, affective *Volk* psyche. Herder's universe pullulated with *Volk* cultures at different stages in their life-cycles; and though he made feeble efforts to establish connexities among them in an overriding concept of *Humanität*, he was the founder of a particular type of German historicism that emphasized the search for concrete psychological "specificity"—the word is his friend Goethe's—in time and place. For both Vico and Herder the nature of things was hidden in the emotional differences among human collectives.

Hegel's contribution to psychological history does not, in my judgment, lie primarily in his characterization of the stages in the history of spirit, but rather in his power to grasp and present the phenomenological fullness of crucial human relationships—for example, his insightful diagnosis of the contrarieties of the master-slave bond, and his depiction of *Entfremdung*, alienation, which was passed on to Marx and Kierkegaard and has since been inflated as the central psychological distinction of modern consciousness.

II

In the last decades of the nineteenth century, there was a substantive discontinuity in the history of psychological history, a breakthrough. Psychology began to achieve a measure of recognition as an autonomous science in the German academic world. Though it had had a long literary and philosophical past—the name itself was invented by Rudolf Goclenius of Marburg in the

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sixteenth century—when psychology first appeared as an independent form of knowledge at the university it underwent a crisis of adolescence. What was it? A physical science in quest of uniformities? Or did it belong to humanist studies and could it be subsumed under history? Almost simultaneously with the rise of experimental psychology, new schools of psychiatry were founded, and the unconscious itself was baptized around the 1880's. This date then becomes a convenient starting-point for taking a prospect of the modern relationship of history and psychology.

In the German and French schools there were two early significant moves in the direction of fusing these disciplines, the older one under Wilhelm Dilthey, born in 1833, and another under Lucien Febvre, born in 1878. Dilthey's first important writing was published in 1883, and twenty-five years later he was still at work on a critique of historical reason, some fragments of which appeared posthumously after World War I.[1] A professor of philosophy in Berlin, he strongly influenced Troeltsch and Meinecke, Heidegger, and a whole generation of German academic historians of ideas. He even left his mark on Spengler, who would have spurned any identification with him. Lucien Febvre ultimately became a professor at the Collège de France and one of the founders of *Annales*, a journal that was to propagate his ideas. Though both Dilthey and Febvre were dedicated to exploring the relationship of psychology and history, they were virtually unaware of each other's existence, separated as were their intellectual worlds by a generation gap and by the then insuperable barrier of the Rhine. And they meant rather different things by psychology. Both, of course, were totally untouched by such outrageous novelties as the doctrines of their contemporary, Sigmund Freud. As for the historical forays of Freud himself and his immediate disciples, they rarely came within the purview of the academic historians of any country at this period. (Preserved Smith, of Cornell University, was one of the exceptions.) Dilthey and Febvre represent two different versions of an initial stage in the emergence of psychological history in recent times.

Though Dilthey's testament remains unfinished, his essays on the towering figures of western European thought, above all his Schleiermacher and his young Hegel, leave no doubt about his purposes.[2] He would have had all studies of man absorbed into intellectual history and would have allowed no independent status to either psychology or sociology. Rejecting the feasibility of any con-

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nected world history in a traditional sense and scornful of the Rankean state as the primary object of historical knowledge, Dilthey was convinced that the history of man could best be presented as a series of psychological world outlooks, more significantly emotive than rationalist, embodied mainly in the writings of literary, religious, and philosophical geniuses. For all his ambition to seize the essence—in a phenomenological sense—of entire ages and to interrelate economico-social and philosophico-religious trends, he always seemed most comfortable with biographical studies of creative men in whom he saw the various psychic currents of an age criss-cross and ultimately assume a manageable structure. His heroic figures are vessels for the dominant passions, cosmic attitudes, and deep-rooted beliefs of a whole epoch. Through the study of the historical varieties of human psychic experience he found an affirmation of freedom, an emancipation from dogmatism, a humanist deliverance. Though he belittled Nietzsche, it is now easy to recognize that with a far less arresting rhetoric and with none of Nietzsche's moral fervor, Dilthey was developing a parallel conception of monumental world personages the expression of whose spiritual, form-imprinting natures was the stuff of history. Resurrecting and consorting with these overmen was virtually the sole justification for historical knowledge.

Rarely, if ever, did Dilthey descend from the heights of exalted intellectual history sprinkled with affects. In theory he was committed to relating the individual psychological natures of his heroes with the grand world outlooks which they had evolved, to lay before us the fullness of his subject's lived experience. But it is always lived experience as a closeted Wilhelmine professor of philosophy conceived it. His histories are elitist dramas of the passions of great men's souls. Dilthey studied the manuscripts of his protagonists and provided us with glimpses of their social status, of their intellectual friendships, occasionally of a great love. Their search for God is always apprehended with a deep sympathy and understanding of the intricacies of the Western religious tradition. But nothing below the navel was mentionable. Economic and social reality may penetrate his narrative, but only as part of a world-view, and political revolutions are quickly transformed into abstract ideas. Incapable of grappling with total psychic breakdown, when confronted by Hölderlin's madness Dilthey dissolves into utter banality and depicts the dying poet as sitting in Tübingen, his mind "wandering, wandering."[3] If this is *Erlebnis*, it is the

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Erlebnis of soap opera. On the other hand, the nuanced description of Schleiermacher's religious experience attains universality. Dilthey was aware of the depths of the unconscious, but for him it was accessible only in the form of an artistic creation.

Dilthey has testified to the impression made upon him by Hüsserl's phenomenology, and in his turn Heidegger in *Sein und Zeit*, which historicized the categories of cognition, described his view of the historical world as dependent on the philosophical implications of Dilthey's writings. To the extent that pure intellectual biography is still being written and there are still attempts to present *Weltanschauungen* in literary psychological terms, Dilthey endures as a living if limited influence. Karl Jaspers, who began his career as a student of psychiatry as practiced in Germany in the first decade of the twentieth century, untouched by Freud, was less restrictive in his studies of Swedenborg, Hölderlin, Strindberg, and Van Gogh, in which he combined a phenomenological psychology with an attempt to communicate the varieties of world historical outlooks.[4] His pathographies were even intended to show the neurotic drives of his characters as allies in their creative achievements, an insight which Freudian psychohistorians have sometimes claimed as their unique discovery. But when he sought to encompass the whole psychic universe in his *Psychologie der Weltanschauungen* of 1919, a work that caused quite a stir in its day, his typologizing remained on the same intellectualist plane as Dilthey's.[5]

The French historical tradition since Michelet has been rich in the study of religious and other forms of emotional expression; but it was not until Lucien Febvre that a declaration was made on the centrality of *histoire psychologique*. Febvre summoned his colleagues to devote themselves to histories of *mentalité* and *sensibilité*. I would loosely translate *mentalité* in this context as what was "thinkable" in a human collective at a given moment of time. While the Germans tended to be impressionistic in their psychological portraits, Febvre insisted on great technical, one might say positivistic, rigor.

The heritage he bequeathed is nonetheless problematic. After we have been assured in the opening of his *Luther* that the underlying preoccupation of his study was the relationship between the individual and the mass, between personal initiative and public necessity, we are left dangling. Whether or not one agrees with the conclusions of Erikson's *Luther*, published as a study in psycho-

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history thirty years later, it does propose answers to the initial query set by Febvre, whose *histoire psychologique* flatly refused to engage with what he dismissed as the hypothetical Luther of the youthful period. "Let us frankly abandon the effort to reconstruct Luther's early surroundings; their effect on his ideas and sentiments could never be estimated . . . It is better even to hold out against the seductions of the psychoanalysts for whose taste no theory is too facile . . . A Freudian Luther is so easy to imagine that one feels not the least curiosity or wish to prosecute the acquaintance when an investigator undertakes to delineate him. For, in fact, might one not with an equal facility conjure up a Lutheran Freud, and observe how completely the illustrious father of psychoanalysis exemplifies permanent traits of the German national genius, of which Luther in his day was so notable an exponent?"[6] This is the voice of a French nationalist of the twentieth century, relying on a fatuous, *ad hominem* argument against Freud, with whose writings he had only the most casual acquaintance.

Febvre's *Rabelais* is the work in which his particular skill in communicating what another epoch "willed, felt, and thought" established the school's prototype of *histoire psychologique*, its virtues as well as its limitations. When Febvre concentrated upon an interpretation of his subject's religious beliefs, he was able to demonstrate with a plethora of empirical evidence that Rabelais' *plaisanteries courantes* and *malices d'Eglise* could be read as proof of atheism only by committing the historical sin of sins—anachronism.[7] Febvre moved in ever-extending circles to define the limits of what was thinkable and what could have been experienced in one's relationship to the supernatural in sixteenth-century Europe. Far from a herald of the new rationalism, Rabelais appears as a kind of Erasmian Christian, as do his giants. In passing, Febvre developed a character for the age in standard literary psychological terms, and he proposed the identification of the discrete elements of a collective historical psychology as the historian's primary mission. But the "mental structure" of the age is confined to the conscious level—the manifest content of ideas and beliefs, the style of their expression, where the line was drawn between the natural and the divine, or the intensity in the externalizing of emotion as compared with men of the twentieth century. In the world of the sixteenth he found imprecision, lack of historical awareness, absorption with the senses of smell and hearing rather than sight. This sort of workmanlike history of ideas and sensibility is still

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practiced in France both in the literary and in the historical faculties, and is yielding a steady flow of respectworthy Gargantuan dissertations on subjects like the idea of nature or of happiness in various epochs.

But Febvre pointed out one of the greatest obstacles in the way of any attempt to define the history of sensibility in a segment of past time. In the course of a review of Johan Huizinga's *Waning of the Middle Ages*, he asked skeptically whether in fact it could ever be determined that some periods were characterized by more love, fear, cruelty, or violence than others in their over-all feeling tone, and he cautioned against reading into other epochs a psychology derived from contemporary sensibility. How would it be possible, he wondered, to apply psychological models of the comfortable twentieth century to ages that knew endemic famine, awoke and went to sleep with the sun, suffered extremes of heat and cold as a norm. He mocked biographies of Pharaohs that were merely portrayals of moderns gotten up in the stage costumes of ancient Egyptians. Cooperative research in which historians and psychologists would be joined—and he referred to French academic psychologists like Dr. Henri Wallon—was the only safeguard he could propose against such follies. But though conscious of the pitfalls, Febvre held to the very end that capturing the unique *sensibilité* of a past age was the ultimate goal of the historian to which all his other efforts were subordinated. "It is true that to presume to reconstitute the emotive life of a given epoch is a task at once extraordinarily seductive and terrifyingly difficult. But what of it? The historian does not have the right to desert."[8] Stirring declamation, *de l'époque*.

One of Febvre's disciples, Robert Mandrou, who adheres to the pure tradition, spent years in the detailed study of the judicial aspects of witchcraft in seventeenth-century France, and has recently produced an exhaustive history to show the transformation in the mentality of the judges of the *Parlement* of Paris that made a belief in witchcraft, still acceptable to so sophisticated an intelligence as Jean Bodin's early in the century, virtually impossible by the end.[9] This is a classical, multifaceted diagnosis of an important change in the mentality of a ruling group and of the political, social, and scientific forces that effectuated the revolution. Mandrou is a superb craftsman, who does not presume to plumb lower depths. His methodology is positivistic and its conclusions hardly to be faulted, except for neglect of the general

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question—which may be the critical one for a historian with a psychological bent in 1970—of what the change signified on an unconscious psychic level. Recent American studies of white racism, perhaps an analogous phenomenon, which sometimes call themselves psychohistorical and may be pretentious and methodologically sloppy, nevertheless give an inkling of how a historian might explore collective obsessions related to excremental and oedipal fantasies.[10] Such ideas are untouchable in the purist French school of *histoire psychologique*, though there has been a growing intrusion of psychoanalytic concepts in recent issues of *Annales*.

When the elaborate outside scaffolding of Michel Foucault's theoretical model is stripped away, there still remain elements of Febvre's original program in Foucault's brilliant and well-documented definitions of seventeenth- and eighteenth-century concepts of madness and reason, and perhaps even in his attempts, in *Les mots et les choses*, to grasp the content of mental structures of various epochs.[11] His analysis is more intricate and formally constructed, if less readily demonstrable, than Febvre's approach to a historical mentality. Freud, who belatedly made the scene in France after World War II, has forced Foucault's generation to peer into the underground recesses that were still forbidden to the previous one. Though I frequently lose Foucault along the way of his argument, I find his work the most exciting new event in French *histoire psychologique*, though not as unrelated to the original master of the school as might be imagined.

III

Apart from the continuation of the Dilthey and Febvre traditions, the years between the World Wars brought novelty in two important respects. First, a number of brilliant young Frenchmen of the Ecole Normale, military class of 1905, Raymond Aron and Jean-Paul Sartre among them, in the most daring intellectual adventure since Mme. de Staël's, traveled to faraway Berlin and submitted themselves to the influence of German sociology and the phenomenological philosophy. At about the same time, the advent of Hitler and a Jewish exodus from Central Europe brought to America men of the generation of around 1900 who had in various ways come within the orbit of Freud's doctrines. I refer to thinkers like Herbert Marcuse and Erik Erikson, not to speak of a large contingent of psychoanalysts who made the United States

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the world center of Freudian thought and practice. Then, in the loose, free-wheeling intellectual atmosphere of the post-World War II period in America, a host of theories were formulated with an intimate though problematic link to Freud. Marcuse tried to amalgamate a philosophical interpretation of Freudian texts—often excerpted ruthlessly without regard to the main body of the work—with a Marxist-Hegelian world-view that on the face of it is totally alien to Freud. Erikson, joining forces with Freudian ego psychologists, struck off in a truly new direction of psychological history, and in the sixties popularized the term "psychohistory." And in the same postwar period Sartre attempted a monumental syncretism in which phenomenological philosophy, Marxist dialectical materialism, and, of late, Freudian psychoanalysis—*quelle galère*—are made to lie together in existential unhappiness. His *Critique de la raison dialectique*, now a decade old, does not yet seem to have made a breach in the historical ramparts of either Europe or America.[12] In France a new generation of structuralists writes about him as if he has long since been, as they say, transcended; but the announcement of his demise is premature.

When Freud finally married the unconscious after the long flirtations conducted by other men (the *mot* is his), the ground was laid for a fundamental innovation in the employment of psychological concepts in history, though, as I have indicated, virtually no historian was aware of it at the time. The therapeutic technique devised by Freud resulted in the accumulation of literally hundreds of thousands of personal histories. Perhaps Erik Erikson exaggerated when he once said that in the twentieth century we have learned more about individual human development than in all previous ages put together; but the clinical histories have surely provided us both with new types of data and with a flood of material that are not of the same order as the literary and philosophical reflections of the past. After all, every classical analysis produces about 10,000,000 words that in some fashion reveal the inner life of a man. From now on out, human conduct can no longer be explained in terms of plain utilitarian motives, as it was by nineteenth-century writers, and even Augustinian churchmen are today willing to complicate the war of the two cities by adopting tactical suggestions on the ways of the devil from the great disbeliever.

Freud himself made a number of applications of psychoanalysis to history. Though he retained a certain diffidence, shying away

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from the interpretation of Descartes' famous dreams, for example,[13] at times he boldly ventured brief psychobiographical hypotheses about creative men, as in his essays on Leonardo and Dostoevsky, where paintings and novels were used as illustrative documents.[14] In this manner he could support his conceptions with objective materials that were public property and did not require disguise, as did his own self-analysis and the case histories of his patients. Aside from his interpretations of literary documents, he invented for us a macrohistorical myth on the origin of civilization; he advanced an extravagant psychological hypothesis about the beginnings of Jewish monotheism (he once called his *Moses and Monotheism* a "historical novel"[15]); and he interspersed his writings with analogies between primitive and neurotic behavior. The identification of phylogeny and ontogeny was axiomatic with Freud. *The psychological history of civilized mankind probably did not* differ substantively from one period to another. Wars and revolutions, whatever their genesis, could be viewed in general terms as changing opportunities for the manifestation of aggression. From time to time there were massive social outbursts against the excesses of instinctual repression in civilized society. But history as a whole meant only the recurrent, eternal conflict of Eros and death.

While Freud's historical essays were attacked for errors in detail, his analytic method of gaining access to the unconscious opened a vast new area to historical inquiry. The followers of Freud, in imitation of the master, but often without his reticence, or at least ambivalence, proliferated psychobiographies. In the beginning their efforts were devoted almost exclusively to the pathography of literary figures—an example would be Marie Bonaparte's *Poe*, which enjoyed the imprimatur of the master in a few introductory remarks.[16] The writings of poets and novelists lent themselves to plausible readings as symbolic representations of the inner states of their authors, their deep loves and hates, their longings and terrors. Fictional incidents were analyzed as disguised materials or fantasy wish-fulfillment of neurotic drives, all by analogy to the dream-work of patients. A similar method of symbolic interpretation was extended to painters by Ernst Kris, a psychoanalyst who was trained as an art historian, and to composers by Editha and Richard Sterba.[17] Except for obiter dicta, until very recently it has not been attempted with physical scientists—though some of us may be creeping up on them, casting doubt on the autonomous development of science itself. The effect

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of all this on historical evidence in the traditional sense is disconcerting, and sometimes constrains the more hide-bound professionals to avert their eyes from the glass, like the eminent contemporaries of Galileo. Henceforth the plainest affirmations in memoirs, dispatches, letters, and secret confessions may require intricate psychological interpretation in the light of the Freudian model. The unconscious demands a hearing and will not be silenced.

Though some psychoanalytic biographers formulated hypotheses about the nature of creativity as a universal phenomenon, in general their artistic subjects were approached as self-contained little monads sufficient unto themselves. This kind of discrete treatment was superseded, however, when political scientists and historians, trying to explain historical developments on the grand scale, applied Freudian personality theory to world-historical actors. An American professor of political science, Harold Lasswell, was one of the first to come up with a formula relating the individual and the collective, something to the effect that the great politician displaces private affects upon public objects.[18] The merit of the political studies has varied enormously; Woodrow Wilson has received a sophisticated treatment at the hands of Alexander and Juliette George and a vulgar one in William Bullitt's analysis, which Freud may or may not have approved.[19] Despite scandalous instances of overinterpretation, immense new vistas have been opened up on the behavior of the monumental figures of world history; and if one is repelled by the grossness of some analyses, one has only to look back on such pre-Freudian compilations as Augustin Cabanès' *Grands névropathes, malades immortels* to appreciate the enormous strides that have been taken.[20]

This, I believe, is the perspective from which to examine the work of Erik Erikson. With Erikson, who has written both programmatic statements and two biographical studies of politico-religious figures, the analytic method applied to history has received its most subtle exemplification.[21] Yet the basic problem raised by Lucien Febvre remains unresolved. We are obliged to ask: Is the ideal psychological model of human development in eight stages, constructed by Erikson, universally applicable? Is this not a summary of twentieth-century psychoanalytic experience, whose relevance to other cultures and periods is open to question?

Clearly, in epochs where the composition of the family, its spiritual and economic character, and life expectancy are very

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different from ours, definitions of the successive crises of life would have to vary. Erikson might say they would need modification. But a historian confronted, for example, with data on Florence 1426-1427 indicating that the average age differential of husband and wife was twenty years and that fathers died early in their children's lives could feel that in this instance the schema would have to undergo drastic alteration.[22] A historian may hold with Plato and Aristotle, Locke and Descartes, yes, and Freud and Erikson that the earliest experiences are far and away the most potent (we remember Descartes' analysis of his predilection for cock-eyed girls based on an early fixation). And he will surely welcome Erikson's shift from exclusive emphasis on infancy and the early family romance to a more extensive view of ego development including periods of life that are better documented and hence more accessible to historical treatment. But can he accept without debate the proliferation of later "crises" in the Eriksonian design, or Erikson's assignment of weight to each of eight crises? The historian should be warned that the selection of materials to fill the boxes of the eight stages may make of the schema a self-fulfilling prophecy. I find wholly credible the crises of adolescence and what early nineteenth-century psychiatrists like Philippe Pinel called the male climacteric, for these are vocal, articulate periods whose anguish is attested by cries and confessions. The rest of the eight stations of life appear rather arbitrary; the traditional divisions of the Church Fathers or Dante's four or Vincent de Beauvais' six may be quite enough. Before I can seriously evaluate the Eriksonian model as a historical tool, I feel that the history of the epigenetic cycle itself, diversified in time and place, needs to be written—and initial soundings in this direction have been made.[23] These reservations aside, however, Erikson's stress upon the total epigenetic cycle, whatever its form, seems to me a permanent acquisition of historical consciousness.

In the two studies of men he calls politico-religious geniuses, Luther and Gandhi, Erikson found a motive drive to heroic action in the need of the sons to outstrip their fathers and compensate for their failures. When he generalizes these conjectures about a small and special sample to all geniuses, he is speaking as a theorist of psychoanalysis, eager to find uniformities, and a historian's judgment must remain suspended. His catalogues of the common characteristics of geniuses—a secret foreboding that a curse lies upon them, a tie to the father which makes open rebellion impossible, a

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sense of being chosen and carrying a superior destiny, a feeling of weakness and shyness and unworthiness, a precocious conscience in childhood, an early development of ultimate concerns, a brief attempt to cast off the yoke of their fate, and a final settling into the conviction that they have a responsibility for a segment of mankind—is troubling even to a historian who has wandered far from the all-too-commonsensical positivists. As a description of Gandhi's and perhaps Luther's experience, yes; as a historical typology, no.

In his Gandhi, Erikson closely scrutinized the manner in which a group of followers resonated to their hero, identified with him in terms of their own life cycles. The skill and imagination with which he handles their recollected dreams, fantasies, and symbolic acts is unmatched in contemporary psychological history-writing. On the other hand, the general relationship between the world historical figure and his age is not advanced much beyond Hegel's lectures. There is no theory of social change in Eriksonian psychology, any more than in Freud's, except for the assumption that each new generation strives to surpass the older one, to innovate upon its works, risking oedipal ambivalence in the process. Erikson does not offer us any help in comprehending either the tempo or the direction of change beyond the categorical assertion that at certain moments mankind is prepared for epoch-making transformations—an epochal identity vacuum is created, he tells us[24] —and the hero comes along and sounds the clarion-call. Each such moment has its potentialities for new creation and redemption from historical psychic blocks that inhibit a society from embarking on a course for which it is ripe and which it desires in its innermost being. The genius leader first releases himself from psychic bondage or points the way to a release for others which he may not have personally achieved.

There is too much of the sacred drama for me in this model of the historical moment. That the hero responds sharply to forces in his world, that he has antennae in his head giving him prescience, foreknowledge, is metaphoric language that German *Zeitgeist* historians have resorted to for many decades. Though he does not mention him, I cannot help hearing the overtones of Hegel's world-historical person, who incarnates the ongoing history of absolute spirit. I am left unenlightened as to what brought about the historical crisis and unconvinced that the hero's prescience is the force that resolves it. On occasion there is a prophetic quality to

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Erikson's heralding of the next stage in world history, with its ever-broadening area of common identities, that leaves the agnostic behind. The austere and rigorous criteria for "psychohistorical evidence" that he set forth in a theoretical statement are not always observed in practice.[25]

Deriving from a totally different tradition is Sartre's existentialist amalgam of psychology and history. He has at least one thing in common with Eriksonian psychohistory, and that is a humanist emphasis upon personality in history and the freedom of, shall we say, ego-will—though Sartre is now prepared to give greater weight to the fetters of inherited psychological conditioning than he was previously.

Sartre renders existential the legacy of a psychologized Marxist framework, fills in the interstices in the history of socioeconomic development with choices of human wills as he presumes Marx would have done, and subjects the action of these wills to psychological analysis. A general Marxist class determinism is postulated, but, in addition, he believes that a humanist history can show how it is possible for individuals with a variety of class and psychic identities to sacrifice their lives in common historic actions. He has fleshed out a Marxist historical dialectic based on production relations and class structure with an existential account of how men who in an inert state have a relationship of mere seriality to one another—like people waiting in a queue—come, in a given historical crisis, to assume ties that entail responsibilities of life and death in action. His convoluted argument is less interesting for a historian than the kinds of questions he addresses to his materials. Rereading histories of the French Revolution, the Revolution of 1848, and the Commune, he asks: What really happens when a crowd moves on an objective? What is the nature of the secret psychological commitments and pacts they have made with one another? How in fact does collective historical action come about? Though Sartre's passion for phenomenological totalization will put many of us off, historians may be able to adapt to other times and places his method of extracting the full existential implications of isolated events. In his treatment of individuals, Sartre has created prototypes for the fusion of social and psychological knowledge. The published fragments of his Flaubert, a work in progress, are a brilliant synthesis of Marxist and Freudian insights.[26]

The penetration of the outer social and ideological world into the intimacy of the family and the psychological history of the

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individual in this primary field of confrontation are for Sartre the first and perhaps most significant of a whole sequence of developments. In the introduction to his *Critique de la raison dialectique*, he criticized his Marxist friends for concerning themselves solely with adults: "Reading them one would believe that we are born at the age when we earn our first wages . . . Existentialism, on the contrary, believes that it can integrate the psychoanalytic method which discovers the point of insertion for man and his class—that is, the particular family—as a mediation between the universal class and the individual."[27] I have considerable sympathy for this viewpoint. At the present time writers who call themselves psychohistorians are forced to use, *faute de mieux*, crude and misleading affirmations about both the economy and the interpsychic relationships of the Western family. A measure of our poverty is the frequency with which Philippe Ariès' history of childhood is cited as Holy Writ.[28] Whenever I have tried to interpret the family relations of historical figures I have felt on shaky ground in relying on old-hat, impressionistic utterances about this nuclear institution. But the possibilities for research are open and the materials cry for exploitation. There seems to be a consensus among social historians, prosopographers, and historians with an interest in psychological phenomena that the history of the family represents a gaping lacuna in our knowledge to which a new generation of historians should give significant priority.

History can now be individuated and particularized in a way that Leopold von Ranke never dreamed of. The economic and social existence of individuals and aggregates today can be seen reflected in the family alongside the psychic pattern set by this primal reality at a crucial period in life. The insights of Marx and of Freud can thus be brought together not in the sibylline macrohistorical rhetoric of Herbert Marcuse or Norman Brown but in concrete historical works where life situations are depicted.

IV

A certain imperialistic character attaches to a major intellectual discovery such as psychoanalysis. When a new instrument for the study of man is developed, believers in its potency tend to conceive of it as a panacea, a solution to a wide range of problems, ultimately including the historical. If it is a successful and persuasive technique that wins assent in one area of the study of man, why

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should it not be introduced elsewhere? Proclamations are issued raising high hopes and staking out large claims. The new technique is often applied with a heavy hand. Revolutionary results are awaited, as it pretends to answer questions for which there already exist explanations more elegant and plausible or more nuanced. It becomes totalitarian. Under attack the proponents of the new technique may withdraw from their most advanced outposts: their position was misunderstood, their theory was not meant to be all-embracing. It is only the most important interpretive device and does not quite account for everything. The original formulation has been misrepresented, misconstrued.

The initial negative reaction of official historians—and there is such a body of academic mandarins in every country—to a new method like psychoanalysis in history is equally fervid. The new technique is based on a series of false assumptions; it is not acceptable even in its own discipline; depending upon it is leaning upon a weak reed. Psychology and history are declared to be different in their essence—as if either of them had an "essence" that enjoys even a partial consensus. The human phenomena that psychological analyses are presumed to illuminate are too elusive, or if they can be grasped they are insignificant and irrelevant as far as the true vocation of the historian is concerned. The evidence brought forth by these techniques is obfuscatory, raising more problems than it resolves—as if the opening up of new problems were not in itself a virtue instead of a fault.

As studies multiply—good, bad, and indifferent—the proponents cite the best as examples and the opponents the most ill-conceived and outlandish specimens. With time, however, both extreme positions are eroded. The imperialists of the new technique pull in their horns, and the absolute deniers of its usefulness permit it a humble place in the republic of knowledge. Eventually the new perceptions insinuate themselves into the most normative orthodox history-writing, often without the author's awareness, and the controversy joins the ranks of those "appearance problems" that at intervals have shaken the intellectual world and that one later reads about with some incredulity. This has been the historical fortune of Marxist conceptions and seems a fair prognosis of what will happen to Freudian ideas in the process of their assimilation by historians.

Despite serious misgivings about some of the uses of psychology in history, I feel we have come a long way from the intellectualist psychological history of the earlier part of the century. I still cast

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my lot with the Freudian "psychologizers." A historian can scarcely compose a narrative line without committing himself, implicitly or explicitly, to some theory of personality and motivation. In various periods since the late seventeenth century there have been dominant psychologies, like those of Locke and Descartes, that seeped into the literary language as well as into everyday speech, and through these media constrained the historian to employ their motivational terminology. Today a historian must feel at least some uneasiness about adhering to the traditional nineteenth-century patterns of motivation. A skeptic may rightly be uncertain that the more novel systems are intrinsically superior or truer in an absolute sense than those handed down by the past; but it is eccentric in 1970 to go about in satin knee-breeches or wear a Prince Albert frock-coat, even if one likes the style. The historian is probably always obliged to accept and express himself in the psychological language of his times; and thus, as I see it, there is no escape from Freud's conceptions in some form, orthodox or heterodox.

Although there may have been a general appreciation of the long interdependency of history and psychology in a wide variety of shapes and forms, on both a theoretical and a practicing level, most members of the American historical profession, positivists and relativists alike, were nevertheless ill-prepared for the bombshell that fell into their midst on December 29, 1957, when William Langer in his presidential address before the American Historical Association in New York announced that the historian's next assignment was an application of the findings of psychoanalysis to history. There were visible stirrings among members of the audience that a behavioral scientist of any school would have identified as consternation. This was the most unkindest cut of all, from a scholar who had produced an impressive array of impeccably solid works of diplomatic history. Since then, there are indications that the stubborn resistance of a phalanx of historians generally suspicious of what they considered random psychological associations is being overcome. Papers dealing with psychology and history, presented at a number of recent meetings of the American Historical Association, have attracted large audiences. In 1963 Bruce Mazlish assembled an impressive group of theoretical statements on psychoanalysis and history, and urged the historian to acquaint himself with the relevant texts.[29] Stuart Hughes reiterated the general sense of Langer's manifesto and added a programmatic statement of his own in *History as Art and as Science* (1964), where he

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called for historical research into the "shared anxieties and aspirations [of various epochs] which may be all the more decisive for being only partially conscious." Experiences such as these, he maintains, cut across the conventional delimitations of class or elite groups, and he has voiced confidence in the feasibility of arriving at valid historical generalizations about "deep-seated fears and ideal strivings."[30] While it is probable that the American historical profession has produced in the last decade more articles about the desirability of establishing a bridge between history and psychology than works animated by the new ideas, that there has been far more talk about the subject than actual performance, some younger historians are beginning to incorporate the findings of the new psychology into the body of their work. For example, the June 1970 issue of the *American Historical Review* contained an article by a young historian, John Demos, "Underlying Themes in the Witchcraft of Seventeenth-Century New England," that was informed by psychoanalytic concepts judiciously introduced.[31] In this respect, however, American historians have lagged behind the political scientists, who have more eagerly embraced the new ideas in their studies of dominant contemporary political figures—witness the *Dædalus* issue on "Philosophers and Kings" (Summer 1968).

The collaborators of the French *Annales,* which has lately shown itself hospitable to articles on psychoanalysis and history, are beginning to explore the psychological aspects of demography, of which they are today the outstanding school in the world. The Russians for Marxist ideological reasons and the official English historians for traditionalist ones are likely to hold out longest against the new trend.

Any contemporary use of psychology in history must postulate the existence of the unconscious, a belief that the unconscious of past epochs has left behind visible traces, and a conviction that these traces are decipherable. About the interpretation of the documentary vestiges of psychic experience there will inevitably be controversy, as there has been over the reading of dead and forgotten languages, and at present the historian is faced with rival and contradictory theories of human development from among which he must choose an initial hypothesis if he is to avail himself of the new techniques. The lack of consensus among psychologists and psychoanalysts is bound to perplex him. Once even a tentative commitment is made, however, to some psychoanalytic theory—and the historian may permit himself the luxury of being

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eclectic and pluralist—the results can vastly enrich our understanding of historical experience. The investigation of psychological phenomena in past epochs not only will alter historical conceptions about other ages, but could lead to an appreciation of the historical dimensions and limitations of present-day psychological doctrines.

Accusations of dilettantism are commonly made against those who are introducing modern psychology into history, and the problem of professional training in two disciplines, psychology and history, is admittedly nettlesome. Yet is it not precisely at the crossroads of two forms of knowledge that the most fertile conceptions in present-day historiography are emerging? The idea of an *équipe*, of the teaming of a psychologist and a historian, is a too-facile solution; an ultimate synthesis must take place within the mind of the historian if the work is to have a wholeness.

On the use of a specialized vocabulary and the borrowing of technical terminology, as contrasted with concepts, my position is rather conservative. I find the psychological jargon, with infrequent exceptions, too ugly for narrative history, and am convinced that one can adopt the concepts without the nomenclature, which derives from a parochial scientistic tradition. After all, no therapist worth his salt will resort in his consultation room to technical terms because of the obvious danger that they may be confused with common and vulgar usage. If the historian eschews technical words, he is less likely to be seduced into dogmatism. His explanations of historical phenomena can be ambiguous, multifaceted, possible, or probable; he can write suggestively, propose solutions by indirection. Psychological labels are unnecessarily restrictive; historical figures are not patients admitted to a hospital who have to be categorized for housekeeping convenience.

Acceptance of psychoanalytic concepts in history-writing presupposes, of course, a somewhat different set of criteria for historical proof than those to which traditional historians are accustomed. For some, the evidence is not concrete enough, and the element of conjecture more obtrusive than they can countenance. While the quantitative school of history continues the Galilean tradition of mathematicizing knowledge with the instruments of a new technology congenial to our age, psychology in history still has a tendency toward the sample of one and the search for uniqueness. Any prospect of quantification seems remote, though not entirely to be excluded as an element in analyses of the behavior of groups.[32]

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On the whole I feel that psychological knowledge is at this stage more useful in description than in explanatory system-making. When two disciplines are locked into the same cage, the historical keepers tell me, cannibalistic tendencies well up from their unconscious and they sometimes try to devour each other. Recent attempts to merge psychology and history in a clumsily labeled psychohistorical process do presume the sort of grand monist thesis to which history has always been refractory. Since so many historico-psychological problems have not been mapped even in a crude manner, I am reluctant to embrace elaborate theoretical structures. In the end I conceive of psychology as playing a more modest role, adding a set of vivid psychedelic colors to the historian's palette, offsetting the mournful black-and-white of structure and number that in this technological age will inevitably suffuse a large part of the historical.

The new psychologies can open up whole areas of inquiry by encouraging the historian to ask some direct, perhaps impertinent questions. The restriction of the method to biography, where it has enjoyed at least partial acceptance, should not be a lasting confinement. Historians will have to wrestle anew with symbolic representation on a broad scale. There are central problems in the history of ideas that cannot be treated adequately on an intellectualist and conscious level of expression alone and that invite the use of new psychological tools: the history of feelings about time, space, utopia, myth, love, death, God. The "unconscious mental habits" that Arthur Lovejoy hoped to analyze in his history of ideas had little or nothing to do with Freud's unconscious. Perhaps the fundamental shortcoming of his work and that of the Febvre school was their exclusive intellectualism. We all know that the experience of love and death and aggression has changed through the centuries. If a professor dared to propose a course on the history of love he would certainly take a ribbing from his colleagues. Yet this emotion has been as protean in its forms and as vital to human existence as, let us say, shipping or banking, whose respectability as subjects for historical treatment in university teaching is quite unchallenged. When a psychologist investigating contemporary attitudes toward death looks for historical comparisons, we can only provide him with impressions and isolated, sparse instances. Particularly dramatic intrusions of death, such as great plagues, have been studied by historians in their psychological as well as their economic, political, and artistic consequences; Millard Meiss's study of the Black Death

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is a prototype.[33] But there is need for an exploration of the long-term, changing meaning of death on the unconscious as well as on the conscious level. Death in the eighteenth century had its special character, but I think we know far less about it than we do about the diplomatic relations between Parma and Venice in that period.

The particular forms of psychic repression have a history and so do the forms of sublimation—their instrumentalities differ widely in time and place. The overt manifestations of neuroses have changed, as any sampling of case histories over the past seventy years will show. As far back as 1913 Karl Jaspers in a section of his *General Psychopathology* entitled "Social and Historical Aspects of the Psychoses and the Personality-Disorders" recognized that "the neuroses in particular have a contemporary style."[34] Few historians have yet coped with the intricacies of presenting to their readers the varying patterns of libidinal satisfaction in different epochs. You can read that grand masterpiece of provincialism, the *Cambridge Modern History,* in its newest version, without even suspecting the existence of these transformations.

The histories of fashion, clothes, sexual and marital customs, punishments, style, and a hundred other questions which have traditionally belonged to *la petite histoire* and to the antiquarians need to be explored for their symbolic content. Freud's second most important legacy to a historian may well be the dissolution of a hierarchy of values among historical materials. If all things can become vehicles of expression for feelings and thoughts, then the state document, grand philosophical affirmation, and scientific law may lose some of their prestige to other more intimate records of human experience. The day of Dilthey's elitist psychological history is over. Conversely, classical psychoanalysis, with a dubious future as a therapy, might be reborn as a historical instrumentality. The dead do not ask to be cured, only to be understood.

A great expansion in our comprehension of the past might be effected through the rereading of old or neglected documents with a different apperception. Notebooks and scribbles that seemed destined for the ashheap of history may be rescued and made to live again—as exciting a reconquest of the past as a new archaeology. A great body of dream literature and of fantasies of past ages is unexplored. In Western Europe and in America there are voluminous materials—legal, political, medical, literary—in manuscript and print, to say nothing of representations by the plastic arts, that have not been researched for their psychological meaning. The

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questions have not been asked, and therefore historians have not been on the *qui vive* for the answers that lie concealed in the texts.

Economic history has become respectable, and who would now demean the history of labor, or the history of consumption patterns? I merely advocate adding to them a history of other needs and expressions of living. In defense of his idealist history of Spirit as the definition of human existence, Hegel once wrote with utter contempt of nutritive history. Now that we have, in despite of Hegel, recognized the claims of hands and stomach to a share in human history, let us make ready to welcome the other more secret and hidden parts of man into the temple.

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ROBERT DARNTON

Reading, Writing, and Publishing in Eighteenth-Century France: A Case Study in the Sociology of Literature

Non numerantur sed ponderantur.
—Marc Bloch[1]

HISTORIANS HAVE always taken what a society writes, publishes, and reads as a guide to its culture, but they have never taken all its books as guidebooks. Instead, they select a few works as representative of the whole and settle down to write intellectual history. Of course those select few may not deserve to serve as cultural attachés. If chosen without proportional representation, they may give a distorted view of reading habits in the past. Nowhere is the culturopomorphic distortion produced by miscast classics more at issue than in the study of the French Enlightenment, a subject located at the crossroads between the traditional history of ideas and more recent trends of social history.

Social historians tend to see the Enlightenment as a social phenomenon—one of the forces of "innovation" opposing "inertia" in the Old Regime (to use the vocabulary of the *Annales* school). They attempt to situate the Enlightenment within a general cultural context rather than to explicate its texts. And they study culture quantitatively, often working from statistics of authorship or book production. This essay will survey their work in order to see what conclusions can be drawn concerning writing and reading in eighteenth-century France and will then attempt to show how that work might be supplemented by an investigation of eighteenth-century publishing. Publishing was an activity where social, economic, and cultural forces naturally converged. But it cannot be understood, in the eighteenth century, without reference to political factors. So a final section will deal with politics and publishing as an aspect of the prerevolutionary crisis.

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I

The quantitative study of eighteenth-century culture goes back to an article published sixty years ago by Daniel Mornet. Mornet tried to measure literary taste under the Old Regime by tallying up titles in five hundred catalogues of private libraries, which had mostly been printed for auctions in the Paris area between 1750 and 1780. He found one lonely copy of Rousseau's *Contrat social.* Eighteenth-century libraries contained a surprisingly small percentage of the other Enlightenment classics, he discovered. Instead their shelves bulged with the works of history's forgotten men and women: Thémiseul de Saint-Hyacinthe, Mme. de Graffigny, and Mme. Riccoboni. Eighteenth-century booklovers divided French literature into "before" and "after" Clément Marot. When they read the philosophes, it was the Voltaire of *La Henriade* and the Rousseau of *La nouvelle Héloise.*[2]

Coinciding ironically with the "great books" approach to the study of civilization, Mornet's research seemed to knock out some of the pillars of the Enlightenment. He made a gap, at least, in the view that the *Social Contract* prepared the way for Robespierre, and his followers have been trying to widen the breach ever since.[3] Meanwhile, the Rousseauists have repaired some of the damage in a counterattack on Mornet's evidence.[4] Why should private libraries important enough to have printed catalogues be taken as an indication of a book's appeal to ordinary and impecunious readers? they ask. They point out that the message of the *Social Contract* could have reached the general reading public through the version of it in book five of Rousseau's highly popular *Emile,* through numerous editions of his collected works, or through editions that came out during the momentous last decade of the Ancien Régime, which Mornet's study did not cover.[5] So Mornet's case remains unproved, either right or wrong.

Nonetheless, Mornet raised some fundamental problems that have only begun to be faced: What was the character of literary culture under the Old Regime? Who produced books in the eighteenth century, who read them, and what were they? It will be impossible to locate the Enlightenment in any cultural and social context until those questions are answered, and they cannot be answered by traditional methods of research.

The most influential attempt to formulate a new methodology has been Robert Escarpit's *Sociologie de la littérature* (Paris, 1958).[6]

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As his title suggests, Escarpit, now director of the Centre de sociologie des faits littéraires at Bordeaux, wanted to define the objects and methods of a new branch of sociology. He treated books as agents in a psychological process, the communication of writer and reader, and also as commodities, circulating through a system of production, distribution, and consumption. Since the author plays a crucial role in both the psychological and the economic circuits of exchange, Escarpit concentrated on the study of writers. They constitute a distinct segment of the population subject to normal demographic laws, he argued, and on this assumption he produced a demographic history of authorship.

In order to survey the literary population, he began with the back pages of the *Petit Larousse,* moved on to bibliographies and biographical dictionaries, and emerged with a list of 937 writers born between 1490 and 1900. He then worked this material into a two-page graph, where the "fait littéraire" appeared in terms of the rise and fall of writers under the age of forty. Escarpit observed that the proportion of young writers rose after the deaths of Louis XIV, Louis XV, and Napoleon. The Edict of Nantes also coincided with an upsurge of youth, which was cut short first after the triumph of Richelieu and then following the collapse of the Fronde. To Escarpit the conclusion was clear: political events determine literary demography. He confirmed this interpretation by reference to England, where the Armada produced a "vieillissement" among writers that was only overcome by the death of James I.

It is a stirring spectacle, this adjustment of the literary population to battles, edicts, revolutions, and the birth of sovereigns. But it leaves the reader confused. Is he to believe that a kind of intellectual contraception took hold of the republic of letters? Did writers limit their population out of loyalty to Good Queen Bess (and Victoria, too), or was *vieillissement* their curse on the queens? Did young men start writing in England in order to make life more difficult for Charles I, or did they stop in France in order to show disaffection for Louis XIV? If one should discount any conscious motivation, why did young writers decrease in numbers after the accession of Louis XIV and increase after the accession of Louis XV and Louis XVI? And why should the birth and death of rulers have such demographic importance—or so much more than the revolutions of 1789 and 1848, which do not disturb the undulations of Escarpit's graph, although 1830 appears as a great turning point?

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The answers to these questions might be found among the deficiencies of Escarpit's statistics. To take 937 writers over 410 years is to spread the sampling pretty thin—an average of 2.3 writers a year. Adding or subtracting a single man could shift the graph by 5 per cent or more, yet Escarpit hung some weighty conclusions on such shifts—his distinction, for example, between a youthful romantic movement and the middle-aged character of literary life under the Empire. More important, Escarpit had no idea of how many writers went uncounted. He evidently believed that a few dozen men (Lamartine and twenty-three others in the case of the early romantics) could represent, demographically, an entire literary generation. A few individuals could, to be sure, represent a new stylistic trend or cultural movement but not the phenomena that can be analyzed demographically, like generational conflict and the adjustment of population to resources.

Escarpit attributed the sociological differences between eighteenth- and nineteenth-century writing to two other factors: "provincialization" and professionalization. He detected a rhythmic "alternance Paris-province" by tracing the geographical origins of his preselected authors. But the geographical argument suffers from the same statistical fallacies as the demographical, and so Escarpit fails to prove that the Paris of Balzac dominated French literature any more than the Paris of Diderot. In the case of professionalization, Escarpit's conclusions seem sounder. He produced two statistical tables to show that there were more middle-class professionals, or writers who lived entirely from their pens, in the nineteenth than in the eighteenth century. But his argument is not helped by the fact that the percentages in the table of eighteenth-century writers add up to 166 per cent.[7]

In this instance, Escarpit drew his statistics from *The French Book Trade in the Ancien Régime* by David Pottinger, another example of the quantitative study of authorship. Pottinger proceeded by combing biographical dictionaries for information about six hundred "writers" who lived between 1500 and 1800. He then sorted his men into five social categories—the clergy, nobility of the sword, high bourgeoisie, middle bourgeoisie, and petty bourgeosie—and apparently concluded that the authors of the Old Regime belonged predominantly to the nobility of the sword and the high bourgeoisie. Again, the conclusion is more convincing than the statistics, because Pottinger destroyed the representativeness of his sample by eliminating 48.5 per cent of the writers on

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the grounds that he could not identify their social background. That stroke of statistical surgery left an average of one author a year to support a social analysis spread out over three centuries. Moreover, Pottinger apparently misfiled many individuals like Restif de la Bretonne, who went into the category of the First Estate because he had a brother who went into the church. Most of the sixteen others in that category either had relatives or protectors who were clergymen. But who in the Old Regime, excepting peasants, did not? Pottinger's other categories are not much more solid. He placed all writers who served in the army or navy with the nobility of the sword and placed teachers, apothecaries, architects, and anyone "whom we can identify with the law or with semilegal positions in the State"[8] in the high bourgeoisie. That kind of admissions policy would put at the top of society many lowly writers who lived like the Neveu de Rameau but called themselves lawyers and even registered with the Paris bar. In any case, it is almost impossible to delimit strata of high, middle, and low bourgeois, because social historians have struggled vainly for years to reach agreement on a meaningful definition of the "bourgeoisie"; and definitions of social stratification in the sixteenth century may not be applicable to the eighteenth.

What then can one conclude from quantitative history's attempts to analyze authorship? Nothing at all. Neither Escarpit nor Pottinger produced evidence to prove that the handful of men they chose to represent the entire literary population of a given period was in fact representative—and neither could possibly do so, because it would first be necessary to have a census of all the writers of the Old Regime. No such census can be contrived, for what, after all, is a writer? Someone who has written a book, someone who depends on writing for a living, someone who claims the title, or someone on whom posterity has bestowed it? Conceptual confusion and deficient data blighted this branch of sociocultural history before it bore its first fruit. But the sociology of literature need not stand or fall on the first attempts to put it in practice. And statistics on reading should be more fruitful than those on writers—if Mornet can be modernized.

II

Mornet showed that a primary obstacle to understanding the culture of the Old Regime is our inability to answer the fundamental

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question: What did eighteenth-century Frenchmen read? The answer eludes us because we have no best-seller lists or statistics on book "consumption" for the early modern period. Quantitative historians therefore have taken soundings in a variety of sources, hoping to tap enough information to reconstruct the general outline of eighteenth-century reading habits. Their predilection for statistics does not imply any belief that they can reduce the reader's internal experience to numbers, or measure quality quantitatively, or produce a numerical standard of literary influence. (Newton's *Principia* would score low on any crude statistical survey.) The quantifiers merely hope to get an over-all view of reading in general and by genre. An enormous amount of data has already been compiled in monographic articles and books by François Furet, Jean Ehrard, Jacques Roger, Daniel Roche, François Bluche (using the work of Régine Petit), and Jean Meyer.[9] Each drew on one of three kinds of sources: catalogues of private libraries, book reviews, and application to the state for authorization to publish. So the reading problem has been heavily attacked on three sides. If it has been cornered, if those long hours in the archives and those laborious calculations have extracted a common pattern from the data, then one can hope to watch the general contours of eighteenth-century literary culture come slowly into focus. Before seeing whether all of the monographs can be synthesized, it is necessary to explain the character of each, because each has special strengths and weaknesses.

François Furet surveyed the Bibliothèque Nationale's registers of requests for permission to publish books. The requests fell into two categories: *permissions publiques* (both *privilèges* and *permissions de Sceau*) for books processed formally through the state's censoring and bureaucratic machinery, and *permissions tacites* for books that censors would not openly certify as inoffensive to morals, religion, or the state. Furet expected that a traditional cultural pattern would show up in the first category and an innovative pattern in the second, because, thanks to Malesherbes' liberal directorship of the book trade, the *permissions tacites* became a paralegal loophole through which many Enlightenment works reached the market during the last half of the century. But what works? How many of them? And in what proportion to the total number of books that can be identified with innovation? Furet could not say. He acknowledged that an unrecorded mass of books circulated with *permissions simples, permissions de police,* and mere *tolér-*

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ances according to the Old Regime's carefully graduated scale of quasilegality. Furthermore, the French stuffed unknown quantities of completely illegal "mauvais livres" into their breeches, the false-bottoms of their trunks, and even the coach of the Parisian lieutenant-general of police. So the official list of *permissions tacites* may not take one very far in identifying innovation.

The identification problems thicken when it comes to classifying the titles entered in the registers. Furet adopted the classification scheme of eighteenth-century catalogues: five standard headings —theology, jurisprudence, history, "sciences et arts," and "belles-lettres"—and a profusion of subcategories that would produce bedlam in any modern library. To rococo readers, travel books belonged under history, and "économie politique" rightly came after chemistry and medicine and before agriculture and agronomy, all happy neighbors in "sciences et arts." But the modern reader is bewildered upon learning that early works on politics (of the *permissions publiques* variety) were "presque tous des manuels de technique commerciale."[10] How can statistics on "économie politique" satisfy his desire to know whether French reading became increasingly political as the eighteenth century progressed? Framing twentieth-century questions within the confines of eighteenth-century categories can be misleading, especially for the researcher trying to fit the Enlightenment into the over-all picture of reading in the Old Regime.

Finally, Furet faced the problem of incomplete data. The requests to print books do not indicate how many copies were printed or the number of volumes, dates, places, and social groups involved in sales. Except in the case of privilege renewals, they give best-sellers the same numerical value as failures—the value of one. They do not even indicate whether a request resulted in an actual publication. And of course they tell nothing about the connection between buying and reading books.

To compensate for these deficiencies, Furet made a broad statistical sweep of the 30,000 titles registered between 1723 and 1789. His analysis of six samplings from the data was thorough enough for him to map out some general trends without professing a detailed knowledge of the eighteenth century's literary topography. He reduced his findings to bar graphs divided into the eighteenth-century categories. The graphs reveal a decline in theological and an increase in scientific writing, which is enough to carry Furet's main conclusion about the "désacralisation" of the world. They also

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reinforce Mornet's belief that the traditional, classical culture inherited from the seventeenth century outweighed the enlightened elements of the eighteenth. But those elements are scattered too haphazardly throughout the graphs to provide any quantitative profile of the Enlightenment.

By quantifying book reviews, Jean Ehrard and Jacques Roger tried to measure eighteenth-century reading by a standard that could not be applied to Furet's data. They attempted to show which kinds of writing had most vogue, as indicated by the number of books reviewed and the length of the reviews in two serious, "quality" periodicals, the *Journal des savants* and the *Mémoires de Trévoux*. They gathered their statistics from approximately the same periods and fit them into the same categories as Furet did, and they came up with complementary conclusions about the rise of interest in science (they locate it earlier in the eighteenth century), the decline of theology, and the "persistance des formes traditionnelles de la littérature."[11] Unfortunately, they made no similar effort to measure their results against Mornet's. Mornet himself had made a careful study of reviews in the *Mercure* and concluded that they bore no relation whatsoever to the real popularity of novels.[12] His findings might be corroborated by more consultation of literary evidence, because eighteenth-century journalism frequently reflected the interests of journalists rather than those of their readers. The journalists of the Old Regime scratched and clawed their way through a world of *cabales*, *combines*, and *pistons* (to use terms that necessity was obliged to invent in the rough-and-tumble French Republic of Letters), and their copy bore the marks of their struggle for survival. Thus the *Journal des savants* featured medical articles very heavily in the early eighteenth century, not because of any great interest among its readers—who actually ceased buying "ce triste répertoire de maladies"—but because the government in effect had taken it over and then surrendered it to a *cabale* of doctors, who used it to propagate their own views on medicine.[13]

Ehrard and Roger tried to cushion their statistics against the shock of such incidents by analyzing a large number of reviews—reviews of 1,800 books in the case of the *Journal des savants*. But it is difficult to winnow conclusions from such data and to coordinate them with other studies. What, for example, can be made of the fact that the *Journal des savants*, a predominantly scientific periodical, reduced its scientific reviewing by almost a third in

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the late eighteenth century? Its reviews showed a decline in the whole category "sciences et arts," while the category "belles-lettres" rose spectacularly. It would be rash to conclude that the public lost interest in science, because the *permissions tacites* showed precisely the opposite trend, according to François Furet. Moreover, a recent study of three other journals by Jean-Louis and Maria Flandrin produced results that contradict both those of Furet and those of Ehrard and Roger.[14] Periodicals do not seem to be a good source for quarrying statistics about the tastes of the reading public.

The catalogues of private libraries, as Mornet originally indicated, might serve quantitative history better. But they present difficulties of their own. Few persons read all the books they own, and many, especially in the eighteenth century, read books they never purchased. Libraries were usually built up over several generations: far from representing reading tastes at any given time, they were automatically archaic. And eighteenth-century libraries were censored for all illegal books before being put up for auction. The censoring may have been imperfect (Mornet found forty-one copies of Voltaire's forbidden *Lettres philosophiques*), but it may also have been influential enough to exclude much of the Enlightenment from the auction catalogues.

Despite these difficulties, Mornet's work remains the most important of its kind, because it covered so many (five hundred) libraries, and because Mornet was able to trace the social position of so many of the owners. He found that they came from a variety of stations above the middle middle-class (a great many doctors, lawyers, and especially state officials, as well as clergymen and nobles of the robe and sword) and that reading tastes did not correlate closely with social status. Louis Trenard got similar results from a nonquantitative investigation of libraries in Lyons.[15] But the most successful applications of Mornet's methods have occurred in studies of a single social group. Daniel Roche's research on the library of Dortous de Mairan actually was limited to the reading of a single man. But Roche made a convincing case for Mairan's typicality as a second-rank savant of the mid-eighteenth century; so his results suggest the general character of reading habits in the influential milieu of lesser academicians. Drawing on the research of Régine Petit, François Bluche studied the libraries of thirty members of the Parlement of Paris, which were catalogued between 1734 and 1795. He worked his findings into a convincing picture of parlementary culture, but not as it evolved over time.

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His comparison of catalogues taken from 1734-1765 and from 1766-1780 does not reveal a declining interest in law and an increased interest in *belles-lettres* and *sciences et arts,* as he maintained, because the statistical differences are trivial—not more than 1 per cent. Nonetheless, Bluche's conclusions correspond quite closely with those of Jean Meyer, who studied the libraries of twenty members of the Parlement of Brittany. Meyer based his statistics on posthumous inventories of property (*inventaires après décès*), which usually are more reliable than auction catalogues as sources. He found a preponderance of "traditional" literature in contrast to a small proportion of enlightened works, and he also noted a decline in the incidence of legal and religious works and an increase in contemporary literature as the century progressed. Quantitative history thus seems to have been instrumental in defining the culture of the high nobility of the robe.

But has it succeeded in measuring the reading habits of France as a whole? There is hope for success in the complementary character of the monographs. Where one is weak, another is strong. Furet surveyed the whole terrain but gave equal weight to every title and did not get near the eighteenth-century reader; Ehrard and Roger got nearer, but their measure of reading incidence seems faulty; Mornet, Roche, and Bluche entered right into eighteenth-century libraries, but only the sections of those libraries that reached public auctions. If each monograph covered the exposed portions of another, the entire topic may be considered safely under wraps. Are the results mutually reinforcing or mutually contradictory? The issue seems important enough to be put graphically (see page 224).[16]

No consistent pattern, unfortunately, can be extracted from this confusing mosaic of graphs. Some of the inconsistencies can be explained away: law naturally shows strongly on the graphs of the *parlementaires,* science on Dortous de Mairan's graph, and theology among the *permissions publiques* as opposed to the *permissions tacites.* But standard categories like *belles-lettres,* history, and science vary enormously; and the proportions are wildly different. By imagining each bar graph as a girl and each black stripe as part of her two-piece bathing suit, one can see what a misshapen, motley crowd of monographs we must live with.

There is some relief from this bikini effect in considering how the monographs spread their proportions over time. They all agree that the French read a great deal of history—so much as to make

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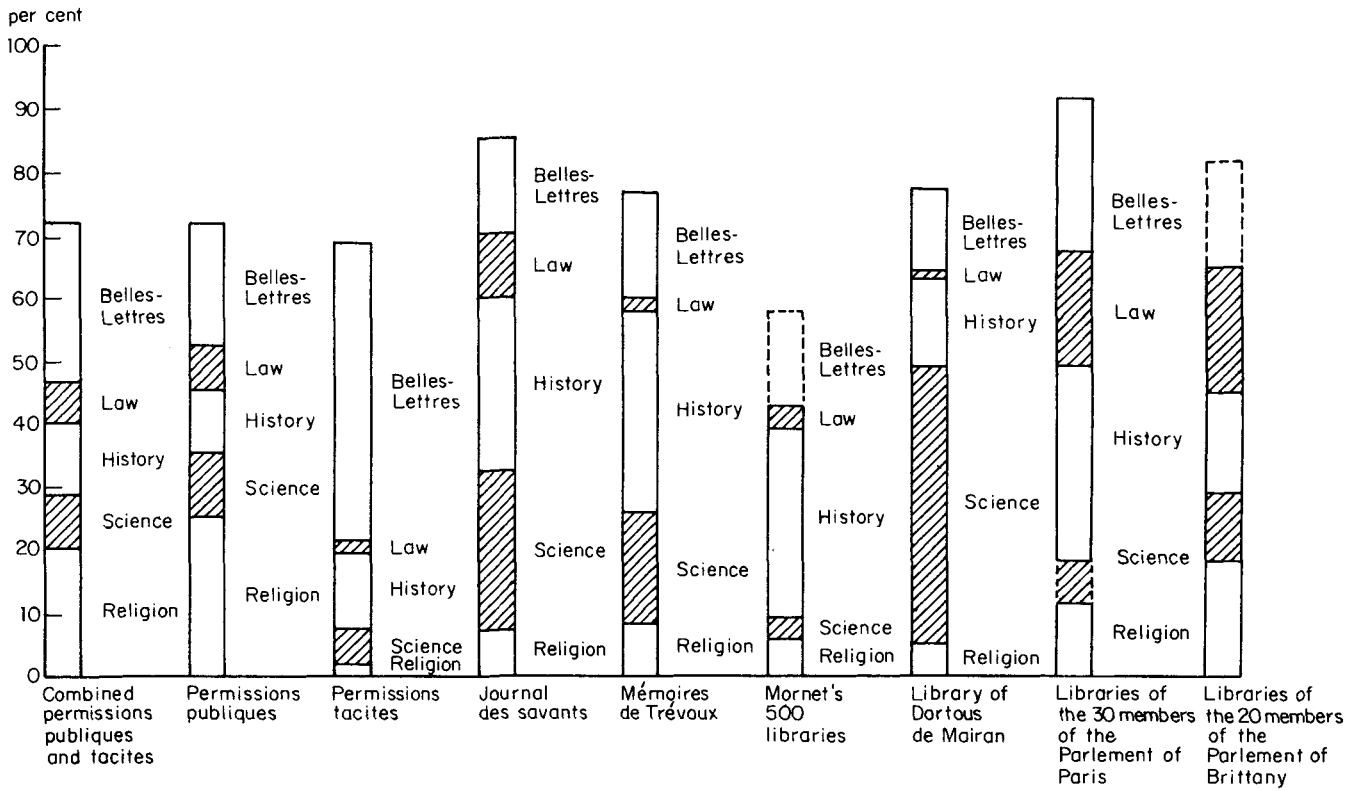
Patterns of Reading in Mid-Eighteenth-Century France



For explanation, see note 16.

untenable the already discredited myth about an "ahistorical" eighteenth century—and read a consistent amount of it throughout the century. The monographs also indicate that the French read less religious literature as time went on. Scientific reading probably increased, although it may have remained constant. And, in general, some "désacralisation," as Furet put it, took hold of the reading public. This tendency, however, might represent an acceleration of a secularizing trend that had begun in the Middle Ages; acknowledging it does not help to refine any generalizations about the Age of the Enlightenment, and no other generalizations can be extracted from the quantitative studies.

Perhaps it is impossible to generalize about the over-all literary culture of eighteenth-century France because there might not have been any such thing. In a country where something like 9,600,000 people had enough instruction by the 1780's to sign their names,[17] there could have been several reading publics and several cultures. In that case, quantitative historians would do better to avoid macroanalysis of reading and to concentrate instead on studies of specific groups like the *parlementaires* of Bluche and Meyer. When used carefully, in conjunction with other kinds of evidence and in reference to clearly-defined segments of the population, this kind of quantitative history has proved to be a valuable tool. But it has not provided answers to the broad questions raised by Mornet, and there is no reason to expect that those answers will emerge from the continued multiplication of monographs.

Just as this essay was going to press, two more statistical studies of eighteenth-century reading were published.[18] They contain another whole series of bar graphs, which are as rich in mutual contradictions as the earlier series. The problem in trying to fit them all into one coherent picture of the Old Regime's literary culture is that they cover different ranges of data: some refer to the reading habits of particular milieux, others to reading throughout France as revealed by different sources. The contradictions are more serious in the second kind, but all of the monographs suffer from deficient data; and the deficiencies will not disappear if more official records and more periodicals are subjected to quantification. The run of graphs could be extended indefinitely. But where will it all lead? Perhaps back to Mornet. No later research has done much either to discredit or refine his emphasis on the mountainous deposits of traditional culture in contrast to the few rivulets of modernity in the literary habits of the eighteenth century.

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But even Mornet's interpretation calls for further proof, because none of the sources examined by him or his successors was likely to contain the most modern works, and none of the categories used for the examining could be considered commensurate with the Enlightenment. The problem of measuring "inertia" against "innovation" in reading during the Old Regime always comes down to a problem of data: to sift statistics through administrative sources, censored journals, or censored library catalogues is to eliminate much of the Enlightenment. No wonder the quantitative historians found the weight of the past so heavy, when so much of the present was excluded from their balance. It may be cruel to conclude that all this laborious quantification has not advanced us far beyond Mornet, but the fact remains that we still do not know much about what eighteenth-century Frenchmen read.

III

If the sociology of literature has failed to develop a coherent discipline of its own, and if its commitment to quantification has not yet produced answers to the basic questions about reading and writing in the past, nonetheless the sociologists and quantifiers have demonstrated the importance of interpreting the Old Regime's literary culture in more than merely literary terms. Books have a social life and an economic value. All the aspects of their existence —literary, social, economic, and even political—came together with the greatest force in the publishing industry of the eighteenth century. So sociocultural history (or the sociology of literature, if the term must be retained) might gain a great deal from the study of publishing. To suggest some of the possible gains, it seems best to draw on material in the papers of publishers and other related sources in order to develop three hypotheses: what Frenchmen read was determined in part by the way in which their books were produced and distributed; there were basically two kinds of book production and distribution in the eighteenth century, legal and clandestine; and the differences between the two were crucial to the culture and politics of the Old Regime.[19]

The differences emerge clearly by a comparison of documents in official archives and those in the papers of clandestine publishers. The bookdealers of Lyons, for example, filled the *Direction de la librairie* with letters and memoranda about their devotion to the law,[20] while addressing the foreign publishers who supplied

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them with illegal books in terms like the following (A. J. Revol, a Lyonnais dealer, is arguing that he did not overcharge the Société typographique de Neuchâtel for his smuggling services):

> Nous avons exposé liberté, vie, santé, argent et réputation.
>
> Liberté, en ce que sans nos amis, nous aurions été enfermé par lettre de cachet.
>
> Vie, en ce qu'ayant été en différentes fois aux prises avec les employés des fermes et les avoir forcés, les armes à la main, à nous restituer les balles qu'ils nous avaient saisies (à cette époque il y en avait douze à votre maison qui auraient été perdues pour vous, sans ressource.)
>
> Santé, combien de nuits avons-nous passé, exposés à toutes les intempéries des saisons, sur la neige, traversé les rivières débordées et quelquefois sur les glaces.
>
> Argent, quelle somme n'avons-nous pas donnée en différentes fois, tant pour faciliter l'exportation que pour éviter les poursuites et calmer les esprits.
>
> Réputation, en ce que nous avions acquis celle de contrebandiers.[21]

Hundreds of men like these operated the underground system for supplying French readers with prohibited and pirated works, the kind that could never qualify for *permissions tacites*. They were colorful characters, these literary buccaneers: the obscure mule-drivers who hauled crates of books over tortuous trails in the Juras for 12 livres the quintal and a stiff drink; the merchants on both sides of the border who paid off the drivers and cleared paths into France for them by bribing agents of the General Tax Farm;[22] the waggoners who took the crates to stockpiles in provincial clearing houses like the Auberge du Cheval Rouge outside Lyons; the provincial bookdealers who cleared the crates through their local guilds (at 5 livres a quintal in Revol's case) and relayed them to entrêpots outside Paris; the entrepôt keepers like Mme. La Noue of Versailles—to all the world a garrulous, warmhearted widow, to her customers a shrewd businesswoman, "passablement arabe"[23] and full of professional pride ("je me flatte que lon sait me randre justice par les precaution que je prand pour cest sorte de marchandises,"[24] she wrote to a client in her semiliterate hand); the colporters like Cugnet et femme, "bandits sans moeurs et sans pudeur"[25] as they were known in the trade, who smuggled the books from Versailles to Paris; and deviate Parisian distributors like Desauges père et fils, who were well acquainted with the Bastille,[26] and Poinçot, "bien avec la police"[27] but "l'être le plus acariâtre que je connaisse,"[28] according to J. F. Bornand, one of the many literary secret agents in Paris who did odd jobs for the

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foreign publishers and completed the circuit by supplying them with manuscripts and best sellers to pirate.[29] An enormous number of illegal books passed through these slippery hands, greasing palms as they went. Their importance in relation to legal and quasilegal literature cannot be calculated until the clandestine import records are compiled. But one nonquantitative conclusion seems significant at the outset: underground publishing and legal publishing operated in separate circuits, and the underground operation was a complicated affair, involving a large labor force drawn from particular milieux. Far from having been lost in the unrecorded depths of history, the individuals who processed clandestine books can be found and situated socially. They had names and faces, which show up vividly in the papers of eighteenth-century publishers. And their experience suggests that underground publishing was a world of its own.

How different was the world of legal publishing. The thirty-six master printers and one hundred or so master booksellers of Paris lived in pomp and circumstance, parading behind their beadle, dressed splendidly in velvet trimmed with gold lilies, on ceremonial occasions; celebrating solemn masses before the silver statue of their patron, Saint John the Evangelist, in the Church of the Mathurins; feasting at the sumptuous banquets held by their confraternity; initiating new members into their guild, a matter of ritualistic oaths and examinations; participating in the Tuesday and Friday inspections of legally imported books delivered to the guildhall by "forts" from the customs and city gates; and minding their own businesses. As businessmen, they kept closed shops. Elaborate regulations—at least 3,000 edicts and ordonnances of all kinds in the eighteenth century alone[30]—specified the qualifications and limited the number of everyone connected with legal publishing, down to the 120 ragged colporters who divided up the official monopoly of hawking almanacs and proclamations in the streets and wore leather badges to prove membership in their corps. Corporateness, monopoly, and family connections tied down every corner of the trade. In fact the cornering of the market dated from a seventeenth-century crisis. In 1666 Colbert had settled a trade war between the Parisian and provincial publishers by, in effect, ruining provincial printing and placing the industry under the control of the Communauté des imprimeurs et libraires de Paris. By ruling this guild, a few families of master printer-booksellers dominated legal French publishing throughout the eighteenth century.

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The guild spirit shows clearly through the major edicts on publishing issued in 1686, 1723, 1744, and 1777. The edict of 1723, which laid down the law throughout most of the eighteenth century, communicates an attitude that might be called "mercantilistic" or "Colbertist," for it codified the reorganization of the trade produced in the 1660's by Colbert himself. Condemning capitalistic "avidité du gain,"[31] it stressed the importance of maintaining quality standards, which it defined in great detail. The type-face of three "l"s must be exactly the same in width as one "m," and the "m" must conform precisely to a model "m" deposited with the syndics and deputies of the guild, who were to inspect the thirty-six printing shops once every three months in order to make sure that each contained the requisite minimum of four presses and nine sets of type, both roman and italic, in good condition. Strict requirements regulated the advancement of apprentices to masterships, which were limited in number and tended to become family possessions—for at every point the edict favored widows, sons, and son-in-laws of the established masters. These privileged few enjoyed an air-tight monopoly of book production and marketing. Non-guild members could not even sell old paper without facing a 500 livres fine and "punition exemplaire."[32] The guild was elaborately organized and favored with "droits, franchises, immunités, prérogatives et privilèges."[33] Not only did it monopolize its trade, but as a corps within the university it benefited from special tax exemptions. Books themselves were tax-exempt. Each contained a formal "privilège" or "permission," granted by the king's "grâce" and registered in the chancellery and in the guild's Chambre syndicale. By purchasing a privilege, a guild member acquired an exclusive right to sell a book, thereby transforming a "grâce" into a kind of commodity, which he could divide into portions and sell to other members. So monopoly and privilege existed at three levels in the publishing industry: within the book itself, within the guild, and as an aspect of the guild's own special status within the Old Regime.

This third level deserves emphasis, because the guild's special position involved a policing as well as an economic function. The state had not often shown an enlightened attitude in its attempts to police the printed word before 1750, when Malesherbes became *Directeur de la librairie*. In 1535 it responded to the discovery that books could be seditious by deciding to hang anyone who printed them. In 1521 it had tried to tame the new industry by subjecting it to the surveillance of a medieval body, the university. And in 1618,

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it tried again, this time by confining publishers within the guild, another rather archaic kind of organization. In addition, the state attempted to bring books under control by developing its own apparatus—at first within the chancellery and the Parisian *lieutenance-générale de police,* later under the *Direction de la librairie*—and by holding its own against rival book-inspectors in the Parlement of Paris, the General Assembly of the Clergy, and other influential institutions. This bureaucratic entanglement did not choke the power of the guild; on the contrary, the guild continued to hunt out "mauvais livres" until the Revolution. The edicts of 1723 and 1777 reaffirmed its authority to search for illegal printing and to inspect books shipped to Paris. This policy made perfect sense: the state created a monopoly with a vested interest in law enforcement, and the monopolists maintained their interest by crushing extra-legal competition. Although some guild members dabbled in underground publishing, most of them wanted to stamp it out. It robbed and undersold them, while the guild existed to protect their privileges. Well-protected privileges meant secure profits, which looked more attractive than the risky business of illegal publishing, especially since illegality exposed them to a double danger: punishment for the particular infraction and then expulsion from the magic circle of monopolists. A printer-bookseller's mastership really belonged to his family. He could not risk it lightly. Better to buy the privilege on a prayer book and to collect a certain but limited profit than to wager everything on a clandestine edition of Voltaire. Such an attitude suited a "traditional" economy, where even merchant adventurers dropped out of trade as soon as they had made enough to invest in *rentes*—or borrowed at 5 per cent to buy land that yielded 1-2 per cent of its purchase price in annual profits.[34]

It would be a mistake, therefore, to underestimate the economic element in the Old Regime's legislation on publishing. P. J. Blondel, an old-fashioned abbé who had no love for philosophes, fulminated against the edict of 1723, even though it tightened the restrictions on philosophic works, because he saw it as a purely economic measure: an extension of the guild's monopoly.[35] Actually, the political and economic aspects of the edict complemented each other. Strengthening the guild seemed to serve the interests of the state as well as those of the privileged publishers. But the reform movement modified the state's view of its interests, and the publishing code of 1777, promulgated soon after Turgot's attacks on

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the six great commercial guilds of Paris, shows a shift away from the old "Colbertism." Instead of condemning "avidité du gain," the king now repudiated any intention of favoring "monopole," praised the effects of "concurrence," and relaxed the rules governing privileges in order to "augmenter l'activité du commerce."[36] He did not undercut the notion of privilege; in fact he confirmed its character as a "grâce fondée en justice"[37] rather than as a kind of property, but he modified it in favor of authors and at the expense of the bookdealers. The guild had tried to prevent such a blow long before it actually struck by getting an author to present its case. The result, Diderot's *Lettre sur le commerce de la librairie,* reiterated the old arguments about maintaining quality by restricting productivity in contradiction to Diderot's own liberal principles and Malesherbes' *Mémoires sur la librairie,* which had partly inspired the reform project. Apparently dismissing Diderot's *Lettre* as the work of a hired hack, Malesherbes' liberal successors, especially Sartine and Le Camus de Néville, pushed through the edicts of 1777 and so somewhat loosened the guild's stranglehold on the publishing industry.[38]

But the controversial item in the code of 1777 concerned the relations of guild members and authors: privilege was now clearly derived from authorship and belonged to the author and his heirs perpetually or expired after his death, if he had ceded it to a bookdealer and the dealer had had it for at least ten years. This provision brought many works into the public domain and provoked bitter complaints by the guild members, but it did not really undermine their monopoly.[39] The code reinforced their power to police the book trade and repeated in the strongest possible terms that no one outside the guild could engage in publishing. So the dynasties of printer-booksellers continued to dominate their industry until the Revolution. The greatest of them, Charles-Joseph Panckoucke, operated as a sort of combination press baron and minister of culture: "sa voiture le portait chez les ministres du roi, à Versailles, qui le recevaient comme un fonctionnaire ayant un portefeuille."[40]

There had never been any question of creating a free trade in books by abolishing the guild as Turgot had abolished the six great *jurandes.* The economic issue took another form; it arose from the ancient enmity between Parisian and provincial bookdealers. Provincial printing had not recovered since the trade war of the seventeenth century, but provincial booksellers survived in large

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numbers throughout the eighteenth century, and they drew much of their stock (often in the form of exchanges, measured in page gatherings) from outside France, where hundreds of enterprising printers turned out cheap pirated editions of French works. The state inadvertently produced a boom in this illicit trade in the 1770's by levying a tax on paper, a much more costly item in the budget of eighteenth-century printers than it is today.

Printer's *papier blanc* had been taxed from time to time, notably in 1680 and 1748, but not at a ruinous rate and not much, if at all, outside Paris—until March 1, 1771, when the abbé Terray, trying desperately to cut the deficit accumulated during the Seven Years' War, taxed it 20 sous per ream. In August 1771 he increased that rate by 10 sous as a result of the across-the-board tax of 2 sous per livre. Since exports of French paper went duty free, foreign printers and their provincial allies gained an enormous advantage. A ream of good white *papier d'Auvergne* cost 11 livres in Paris and 8 livres in Switzerland, according to one estimate.[41] To right the balance, Terray placed a duty of 60 livres per quintal on imports of French and Latin books on September 11, 1771. But this measure massacred the exchange trade between provincial dealers and foreigners.

Seized by panic, publishers like the Société typographique de Neuchâtel suspended all shipments to France and cast about desperately for ways of cracking the tariff barrier while their tough customers in the provinces, men like Jean Marie Bruysset and Périsse Duluc of Lyons, agitated for the repeal of the duty.[42] The agitation paid: on November 24, 1771, the tax was reduced to 20 livres; on October 17, 1773, it went down to 6 livres 10 sols; and on April 23, 1775, Turgot withdrew it altogether. But this reversal of policy again tipped the economic balance in favor of the foreign publishers. An unsigned memorandum to the ministry reported: "C'est depuis ce moment que les Suisses, ayant senti qu'ils pouvaient donner nos livres à 50% meilleur marché que nous, ont pillé et ravagé notre librairie, et en effet ils donnent nos livres a trois liards ou un sol de France la feuille, et comme aux frais de l'impôt sur le papier, du haut prix de l'impression et du tirage en France, il faut joindre l'achat des manuscrits, on ne peut souvent pas trouver de bénéfice en vendant cette même feuille deux ou trois sols." As an example, the writer said that Panckoucke's new *Encyclopédie méthodique* would have to sell at 11 livres a volume for Panckoucke merely to cover production costs, while a pirated

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Swiss edition could sell in Paris at 6 livres a volume and produce a 40-50 per cent profit.[43]

Until mid-1783 the business of foreign publishers and provincial dealers seems to have flourished at the expense of their Parisian rivals, but on June 12, 1783, Vergennes, the foreign minister, destroyed it with a stroke of the pen. He issued orders to the General Tax Farm requiring that all book imports—garnished with the usual seals, lead stamps, and *acquits à caution*—be transmitted to the Chambre syndicale of the Parisian guild for inspection before being delivered to their final destination. Without tampering further with the taxation system or passing through formal, legal channels like the earlier edicts, this measure at once restored the guild's domination of the book trade. It meant that a crate of books sent from Geneva to Lyons now had to pass through the hands of the guild officials in Paris, which gave the Parisians an opportunity to weed out pirated editions and saddled the Lyonnais with a detour that would cost more than the books were worth. Even the extra trip from Rouen to Paris and back would ruin his business, a desperate Rouennais wrote.[44] Booksellers in Lille reported that they had no choice but to let imports pile up and rot in their damp customs house.[45] The Lyonnais claimed that they had suspended all book imports—a matter of 2,000 quintals a year—and were in danger of suspending payments.[46] And while protests from provincial dealers flooded the *Direction de la librairie*, frantic letters flew around the circuit of publishers who fed the provincials from across France's borders. Boubers of Brussels, Gosse of The Hague, Dufour of Maestricht, Grasset of Lausanne, Bassompierre of Geneva, and dozens of others, all trembled for their commercial lives. The Société typographique de Neuchâtel sent out an agent, J.-F. Bornand, to inspect the damage done to its supply lines. Bornand reported that the "malheureux arrêt" had stopped all book traffic in Savoy and Franche-Comté. A side trip to Grenoble showed him that the southern route was "hérissée de gardes, au point qu'au bureau de Chaparillan on m'a saisi tous mes livres dans ma malle . . . en nous faisant voir l'ordre du roi qui leur enjoint de ne laisser passer aucune librairie quelconque."[47] The bookdealers in Lyons told Bornand such gloomy stories that he concluded, "Il faut renoncer à la France."[48] They believed that Panckoucke was behind the crackdown, because he wanted to destroy his Swiss competitors, notably Heubach & Cie, of Lausanne, whose pirating had cut deeply into the sales of his edition of Buffon's *Histoire*

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naturelle. Bornand reported the same rumor from Besançon; and when he arrived in Paris, the booksellers turned their "air de mépris" on him with full force. One threatened to cause him "tout le mal possible, et c'est un pacte formé entre les libraires de Paris contre les libraires étrangers et même contre ceux de province."[49] By mid-1785, the Neuchâtelois still found it impossible to get their books to the great clandestine trade center of Avignon,[50] and they abandoned attempts to reach Paris through smugglers stationed in Geneva, Besançon, Dijon, Châlons-sur-Saône, and Clairvaux. Their booming business in France had been cut to a trickle. It never recovered, because, as they explained to a Parisian confidant, "Nous ignorons de quelle voie se servent les autres imprimeurs d'ici, de Lausanne et de Berne; nous ne connaissons point d'autre que d'expédier sous acquit à caution pour Paris . . . Toute autre voie nous est interdite, parce que nous ne voulons pas courir des risques, ni nous exposer à la confiscation et à l'amende."[51] Vergennes had cut the lifeline linking foreign producers and provincial distributors.

According to the provincial protests, Vergennes' orders would decimate the legal foreign trade in books. By making imports impossibly expensive, the new rules would produce an inevitable decline in exports, especially since the import-export business was usually conducted in exchanges of so many page-gatherings rather than in money. The state saw the orders as a new policing technique, aimed at the destruction of pirated and prohibited books—the bread and butter of underground publishing. Both views may have been correct, but the clandestine trade probably suffered the most. The monopolistic practices of the Parisians had forced the provincials to seek shelter underground. There they formed alliances with foreign publishers, who sent them illegal works under cover of an *acquit à caution*, a customs permit that protected book shipments from all inspection between the border and their points of destination within France, where they were to be examined by the nearest official bookdealer. He would certify their legitimacy by endorsing the back of the *acquit* and returning it, by the driver who had delivered the books, to the border station where it had been issued. A dealer collaborating with an illegal publisher could either market the books himself (instead of impounding them), or he could relay them on toward Paris and collect a commission. Since domestic book shipments were never inspected en route, they could reach an entrepôt outside Paris, usually in Versailles, without risk

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and then could be smuggled in small quantities into the capitol. The system worked quite well as long as provincial dealers could discharge the *acquits à caution*. But by placing that function in the hands of the Parisian guild, Vergennes undercut the whole operation. Of course there were other ways of reaching the market, but it was no easy task to thread one's way through the internal customs barriers and to dodge the roaming inspectors of the General Farm, who received a reward and a portion of the goods after every confiscation. What the drivers and clandestine agents wanted was legal camouflage so they could send whole wagonloads rumbling down the middle of France's splendid highways to provincial guildhalls and to the very palace of the king. The clandestine trade was a matter of calculating risks and profit margins. Too chancy, too elaborate a system of smuggling would not pay. So when Vergennes changed the rules of the game, the foreign suppliers and provincial dealers faced disaster. If the papers of the Société typographique de Neuchâtel indicate the general reaction to the order of June 12, 1783, the whole underground industry fell into a depression that lasted for at least two years and perhaps until 1789.[52] As far as foreign publishing was concerned, the French government had finally committed itself to a policy of laissez faire but not laissez passer.

Curiously, the graphs of legal French book production constructed by Robert Estivals and François Furet also show a spectacular drop in 1783, the low point of a slump extending roughly from 1774 to 1786.[53] Why this slump occurred is difficult to say. It does not seem to be related to Labrousse's prerevolutionary economic crisis or the Labrousse-like "cycles" that Estivals somehow sees in his statistics. Could it be connected with Vergennes' orders of June 12, 1783? The purpose of the orders stands out clearly in the text: to put an end to "la multitude de libelles imprimés dans l'étranger et introduits dans le royaume."[54] Even the petitions from the provincial bookdealers acknowledged that "le motif de l'ordre est d'empêcher l'introduction des libelles qui viennent de l'étranger."[55] And a glance at Vergennes' correspondence with his ambassadors shows how much the *libelles* concerned him. In 1782 and 1783 he wrote as many letters to England about the need to suppress a smut factory run by émigré French "libellistes" as he did about the diplomatic preliminaries to the Treaty of Paris. He sent secret agent after secret agent (a bizarre collection of bogus barons and one police inspector disguised as an umbrella salesman) to buy

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off or kidnap the *libellistes.* No details of their fantastic, rococo intrigues were too trivial for Vergennes' attention, for he feared the effect of the *libelles* on public opinion in France. Well before the Diamond Necklace Affair, he exhorted the French chargé d'affaires to stamp out political pornography: "Vous connaissez la malignité de notre siècle et avec quelle facilité les fables les plus absurdes sont reçues."[56] The orders of June 12, 1783, must have been part of this campaign, and they must have been fairly successful, judging from the consternation they produced in the world of underground publishing and the large collection of works like *Les amours de Charlot et Toinette* and *Essais historiques sur la vie de Marie-Antoinette* that the revolutionaries gleefully inventoried in the Bastille after 1789.[57]

There is no reason to connect the campaign against *libelles* with the drop in legal book production. Nonetheless, it seems possible that Vergennes was so determined to shut off the flow of *libelles* from outside France that he dammed up the channels of legitimate imports, too. His action could have created repercussions in the legal system of publishing, exactly as the provincial dealers argued. It would have forced even the most honest provincial booksellers to retrench, because it would have increased their expenses drastically and destroyed their exchange trade. It would also have eliminated their roles as middlemen (an important business in Lyons) in commerce between northern and southern Europe. As always, the Parisians might have profited from the provincials' losses. But provincial dealers drew some of the stock that they used for foreign exchanges from Paris. So Vergennes' offer also could have damaged part of the Parisians' market. It certainly reduced book imports on a national scale and, owing to the crucial importance of exchanges in the book trade, probably produced a corresponding drop in exports. Over-all French book production therefore would have suffered, just as it suffered from the buffeting given it since 1771 by the succession of taxes and tariffs. If these hypotheses are correct, they suggest that underground and legal publishing were not so separate and so inimical that they could not be injured by a common blow. A certain symbiosis might have attached segments of the two circuits. Each circuit relied heavily on injections of foreign books, and that foreign element must be measured if there is to be more exact knowledge about the circulation of ideas in the Old Regime. At this prestatistical stage, however, it seems legitimate to insist on one point: far from flourishing as a result of

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virtual freedom of the press, as is usually maintained, French publishing underwent a severe crisis on the eve of the Revolution, a crisis that has not been noticed by historians, because it did not manifest itself in formal documents, like the edicts on the book trade.[58]

The publishing crisis seems especially worthy of notice, because its economic and intellectual aspects were related in a way that reveals aspects of the prerevolutionary crisis. Economically, legal and clandestine publishing stood for antithetical ways of doing business. Faithful to the old "Colbertist" methods, the Parisian Communauté des libraires et imprimeurs produced a limited number of quality goods according to official specifications. It turned out traditional books for a traditional market, which it controlled by virtue of an official monopoly. It ran no risks, because it owed its profits to its privileges; and its privileges were family treasures, handed down from father to son and husband to widow. Furthermore, the guild fortified its monopoly by a share in the repressive power of the state. In publishing, as in so many other cases, the Old Regime was eaten away by privilege—not the juridical privileges dividing nobles from commoners, but the privilege of vested interests, which devoured the state like a cancer. In its last years, the government tried to rally and reform. But its efforts reactivated the century-old conflict between provincial and Parisian bookdealers, and the book duties of 1771-1775 followed by Vergennes' order of June 12, 1783, represented the final triumph of the Parisian publishing dynasties.

But this triumph was limited by the limitations of an archaic production system. Despite the flexibility introduced through the use of *permissions tacites* and the adventurous policies of a few guild members, privileged publishing failed to satisfy the demand created by an enlarged readership and by changing literary tastes. The reading patterns of the past weighed heavily in the traditional sector of publishing, as the statistics of Mornet and Furet demonstrate; and the reluctance of most traditional publishers to deviate from those patterns is perfectly understandable. Why should they abandon their privileges, risk their special status, and endanger their families' livelihood by producing new literature of uncertain legality? "Innovation" came through the underground. Down there, no legalities constrained productivity, and books were turned out by a kind of rampant capitalism. Not only did the state's misguided fiscal policies make it cheaper to produce new works outside

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France, but foreign publishers did a wild and woolly business in pirating old ones. As soon as their agents reported that a book was selling well in Paris, they began setting type for a counterfeit edition. Some of them also printed prohibited, hard-core "mauvais livres." They were tough businessmen who produced anything that would sell. They took risks, broke traditions, and maximized profits by quantity instead of quality production. Rather than try to corner some segment of the market by a legal monopoly, they wanted to be left alone by the state and would even bribe it to do so. They were entrepreneurs who made a business of Enlightenment.

The enlightened themes of the books they produced—individualism, liberty, and equality before the law as opposed to corporatism, privilege, and "mercantilist" restrictions—suited their way of doing business. A Marxist might argue that the modes of production determined the product—an extravagant interpretation, but one that might serve as an antidote to the conventional history of ideas.[59] Books are economic commodities as well as cultural artifacts; and as vehicles of ideas, they have to be peddled on a market. The literary marketplace of eighteenth-century France calls for closer analysis, for its books—whether privileged or philosophic, traditional or innovative—epitomized the character of the Old Regime.

IV

Since the Old Regime was a political as well as a social and economic system, a socioeconomic interpretation of its publishing ought to take account of political factors. What, in fact, were those books that Vergennes wanted so desperately to keep out of France? They were listed in handwritten catalogues entitled "livres philosophiques," which circulated secretly and offered such delicious forbidden fruit as:[60]

Vénus dans le cloître, ou la religieuse en chemise, figures
Système de la nature, 8°, 2 vol. 1775 très belle édition
Système social, 8°, 3 vol. 1775
Fausseté des miracles
La fille de joie, 8°, figures
Contrat social par Jean-Jacques Rousseau 12°
Journal historique des révolutions opérées en France par M. Maupeou, 3 vol. 8°
Mémoires authentiques de Mme. la comtesse Du Barry, 1775
Margot la ravaudeuse, 12°, figures

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Lettres de l'abbé Terray à M. Turgot
Les droits des hommes et leurs usurpations

The same underground publisher also circulated a formal printed catalogue, openly advertising its name, address, and items like the following:[61]

Bélisaire, par Marmontel, nouvelle édition augmentée, 8°, figures, Lausanne, 1784: 1 livre.
Bible (la Sainte), 8°, 2 vol., Neuchâtel, 1771: 6 livres.
Bibliothèque anglaise, ou recueil des romans anglais, 14 vol., 12°, Genève, 1781: 15 livres.
Bonnet (M. Charles), ses oeuvres complètes de physique et d'histoire naturelle, 4°, 8 vol., figures, Neuchâtel, 1782: 81 livres.

The books in the second catalogue may have been legal or pirated, but they did not offend religion, morality, or the French state. Those in the first catalogue offended all three and therefore earned the title "livres philosophiques"—a very revealing trade name, which recurs constantly in the commercial correspondence of underground publishers.

How offensive actually was this "philosophy"? *Les amours de Charlot et Toinette*, a work that was high on Vergennes' list of *libelles*, began with a description of the queen masturbating and then moved on to an account of her supposed orgies with the comte d'Artois, dismissing the king as follows:[62]

On sait bien que le pauvre Sire,
Trois ou quatre fois condamné
Par la salubre faculté,
Pour impuissance très complète,
Ne peut satisfaire Antoinette.
De ce malheur bien convaincu,
Attendu que son allumette
N'est pas plus grosse qu'un fétu;
Que toujours molle et toujours croche,
Il n'a de v . . . que dans la poche;
Qu'au lieu de f . . . il est f . . .
Comme le feu prélat d'Antioche.

Crude stuff, but no less ineffective for its gross versification. A similar work, which pretended to defend the queen, and various courtiers and ministers as well, by refuting the calumnies against her in minute, scabrous detail, explained that the *libelles* circulated through several strata of society:[63]

Un lâche courtisan les ["ces infamies"] met en vers en couplets, et, par le ministère de la valetaille, les fait passer jusqu'aux halles et aux marchés

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aux herbes. Des halles elles sont portées à l'artisan qui, à son tour, les rapporte chez les seigneurs qui les ont forgées, et lesquels, sans perdre de temps, s'en vont à l'Oeil-de-Boeuf se demander à l'oreille les uns aux autres, et du ton de l'hypocrisie la plus consommée: Les avez-vous lues? les voilà. Elles courent dans le peuple de Paris.

No doubt one could pick up some smut from the gutter at any period in the history of Paris, but the gutters overflowed during the reign of Louis XVI; and the inundation worried Louis' chief of police, J.-C.-P. Lenoir, because, as Lenoir put it, "Les parisiens avaient plus de propension à ajouter foi aux mauvais propos et aux libelles qu'on faisait circuler clandestinement, qu'aux faits imprimés et publiés par ordre ou permission du gouvernement."[64] Lenoir later reported that his attempt to suppress the circulation of *libelles* "furent combattus par des hommes de la cour qui faisaient imprimer ou protégeaient l'impression d'écrits scandaleux. La police de Paris ne pouvait atteindre que les marchands et colporteurs les vendant et débitant. On faisait enfermer les colporteurs à la Bastille, et ce genre de punition ne mortifiait pas cette classe de gens, pauvres mercénaires qui souvent ignoraient les vrais auteurs et imprimeurs . . . C'est surtout à l'égard des libelles contre le gouvernement que les lois, dans les temps qui ont précédé la révolution, furent impuissantes."[65] The police took the *libelles* seriously, because they had a serious effect on public opinion, and public opinion was a powerful force in the declining years of the Old Regime. Although the monarchy still considered itself absolute, it hired hack pamphleteers like Brissot and Mirabeau to give it a good name.[66] It even attempted to manipulate rumors, for eighteenth-century "bruits publics" produced eighteenth-century "émotions populaires"—riots. A riot broke out in 1752, for example, because of a rumor that the police were kidnapping working-class children to provide a literal blood bath for some royal prince of the blood.[67] It was the primitiveness of such "emotions" and opinions that made the regime vulnerable to *libelles*.

How badly the *libelles* damaged the public's faith in the legitimacy of the Old Regime is difficult to say, because there is no index to the public opinion of eighteenth-century France. Despite the testimony of expert observers like Vergennes and Lenoir,[68] it might be argued that the public found its dirty books amusing, nothing more. *Libellistes* had piled up trash for years without burying anyone. But there also could have been a cumulative effect that produced a deluge after Louis XV. Louis' private life pro-

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vided plenty of material for the *Vie privée de Louis XV*, which in turn set the tone for a whole series of *Vie privées* about court figures. These scurrilous works hammered at the same points with such ferocity that they probably drove some home, at least in the case of a few leitmotivs: Du Barry's sexual success story (from brothel to throne), Maupeou's despotism (his search for a man to build a machine that would hang ten innocent victims at a time); and the decadence of the court (not merely a matter of luxury and adultery but also of impotence—in the *libelles* the high aristocracy could neither fight nor make love and perpetuated itself by extramarital infusions from more virile lower classes).[69] Louis XVI, notoriously unable to consummate his marriage for many years, made a perfect symbol of a monarchy in the last stages of decay. Dozens of pamphlets like *La naissance du Dauphin dévoilée* (another on Vergennes' list) provided dozens of revelations about the real lineage of the heir to the throne. And then the Diamond Necklace Affair produced an inexhaustible supply of muck to be raked. A king cuckolded by a cardinal: What better finale to a regime that was finished—better even than the rumor of the warming pan that brought public opinion to a boil in England on the eve of 1688.

It is easy to underestimate the importance of personal slander in eighteenth-century French politics, because it is difficult to appreciate that politics took place at court, where personalities counted more than policies. Defamation was a standard weapon of court cabales. And then as now, names made news, although news did not make the newspapers. Rigorously excluded from legal periodicals, it circulated in pamphlets, *nouvelles à la main*, and by *nouvellistes de bouche*—the real sources from which political journalism originated in France. In such crude media, politics was reported crudely—as a game for kings, their courtiers, ministers, and mistresses. Beyond the court and below the summit of salon society, the "general public" lived on rumors; and the "general reader" saw politics as a kind of nonparticipant sport, involving villains and heroes but not issues—except perhaps a crude struggle between good and evil or France and Austria. He probably read his *libelles* as his modern counterpart reads magazines or comic books, but he did not laugh them off; for the villains and heroes were real to him; they were fighting for control of France. Politics was living folklore. And so, after enjoying *La gazette noire's* titillating account of venereal disease, buggery, cuckoldry, illegitimacy, and im-

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potence in the upper ranks of French society, he may have been convinced and outraged by its description of Mme. Du Barry[70]

passant sans interruption du bordel sur le trône, des bras des laquais dans ceux du monarque; culbutant le ministre le plus puissant et le plus redoutable; opérant le renversement de la constitution de la monarchie; insultant à la famille royale, à l'héritier présomptif du trône et à son auguste compagne, par son luxe incroyable, par ses propos insolants, à la nation entière mourant de faim, par ses profusions vaines, par les déprédations connues de tous les roués qui l'entouraient; voyant ramper à ses pieds non seulement les grands du royaume, les ministres, mais les princes du sang, mais les ambassadeurs étrangers, mais l'Eglise canonisant ses scandales et ses débauches.

This was more dangerous propaganda than the *Contrat social.* It severed the sense of decency that bound the public to its rulers. Its disingenuous moralizing opposed the ethics of little people to those of "les grands" on top, because, for all their obscenities, the *libelles* were strongly moralistic. Perhaps they even propagated a "bourgeois morality" that came to full fruition during the Revolution. "Bourgeois" may not be the proper term for it, or for the Revolution either, but the "petits" who rose against the "gros" in the Year II responded to a kind of Gaulois Puritanism that had developed well before 1789. Gullible about the plots and purges of the Terror,[71] they had gullibly assimilated legends from their earlier *libelles.* Thus an aristocratic plot to kidnap bourgeois wives before the Revolution: "Avez-vous une jolie femme? Est-elle du goût de quelque nouveau parvenu, de quelque petit fat en puissance, de quelque talon rouge, par exemple? On vous la séquestre proprement. Voulez-vous raisonner? On vous envoie aux galères."[72] Of course one can only speculate about what went on in the minds of such primitive readers, but it might have been "désacralisation," occurring at levels well below the elite. Without this occurrence, it is hard to understand how the *Père Duchesne* could have had such an appeal or how people brought up to believe in the royal touch could have read about "la tête de véto femelle séparée de son foutu col de grue"[73] without erupting in "émotion populaire." The king had lost some of his mystical touch with the people long before Hébert's harangues about the "louve autrichienne" and her "gros cocu." How great a loss it was, no one can say, but works like *Les rois de France régénérés* made the Bourbons look literally illegitimate. The administration feared those works, because it appreciated their power to make a mockery of the monarchy. The ridiculing of Louis XVI

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must have done a great deal of damage at a time when nobility was still identified with "liqueur séminale"[74] and when the Salic Law still required that the royal "race" be transmitted through a magical unbroken chain of males. The magic had gone out of the Bourbons by the reign of Louis XVI. Lenoir reported that as the Revolution approached he could not get crowds to applaud the queen by paying them, although they had cheered spontaneously earlier.[75] And in 1789 Desmoulins described a four-year-old being carried around the Palais-Royal on the shoulders of a street-porter, crying out, " 'La Polignac exilée à cent lieues de Paris! Condé idem! Conti idem! d'Artois idem! la reine . . . !' je n'ose répéter."[76] The *libelles* had done their work all too well.

The step from publishing to libeling was easily taken outside the closed circles of the guild because nonguild publishers could only exist outside the law, and law in the Old Regime meant privilege (*leges privatae*, private law).[77] The nuances of legality and illegality covered a broad enough spectrum, however, for many underprivileged bookdealers to do a pretty legitimate business. The underground contained several levels. Its agents near the top may never have touched *libelles*, while those at the bottom handled nothing but filth. The Société typographique de Neuchâtel generally pirated only good, clean books like the works of Mme. Riccoboni, but the neighboring house of Samuel Fauche and his prodigal sons produced the very works that Vergennes tried to suppress in London. Fauche also printed the political and pornographic writings of Mirabeau: *l'Espion dévalisé, Ma conversion ou le libertin de qualité, Erotika Biblion*, and *Lettres de cachet*.[78] And yet when the last ten volumes of the *Encyclopédie* appeared in 1765, they bore the false imprint "A Neufchastel chez Samuel Faulche." The underground genres easily got mixed up, and underground dealers often moved from one level to another. Hard times forced them into lower reaches of illegality; for as they sank deeper into debt, they took greater chances in hopes of greater profits. The crisis of the 1780's might have produced precisely that result. Ironically, Vergennes might have transformed some rather inoffensive pirates into purveyors of *libelles* and actually increased the circulation of "livres philosophiques" by decreasing the relatively above-the-board traffic in *contrefaçons*. The Société typographique de Neuchâtel seems to have done more business in *libelles* after 1783 than before Vergennes' crackdown.[79] As the Revolution approached, provincial dealers who earlier had merely

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discharged a few false *acquits à caution* may have speculated more on shipments of works like *Les amours de Charlot et Toinette* and passed around more catalogues of "livres philosophiques." Or perhaps their customers' tastes changed in response to episodes like the Diamond Necklace Afair. It is impossible at this point to tell whether supply followed demand fairly neatly or whether demand was influenced by what could be supplied. Reading habits could have evolved as a result of the peculiar conditions determining literary output or could have been the determining factor themselves; or each element could have reinforced the other. Whatever combination of causes was at work, the Old Regime put *Charlot et Toinette, Vénus dans le cloître,* d'Holbach, and Rousseau in the same boxes and shipped them under the same code-name. "Livres philosophiques" to the dealers, "mauvais livres" to the police, it made little difference. What mattered was their common clandestineness. There was equality in illegality; Charlot and Rousseau were brothers beyond the pale.

The very way in which these works were produced helped reduce them to the common denominator of irreligion, immorality, and uncivility. The foreigners who printed them felt no loyalty to France, the Bourbons, or, often, the Catholic Church. The dealers who distributed them operated in an underworld of "bandits sans moeurs et sans pudeur." And the authors who wrote them had often sunk into a Grub Street life of quasi-criminality. The arch-*libelliste* Charles Théveneau de Morande was brought up in brothels and educated in prisons, and those mileux provided the material for his writing.[80] Perhaps the underground's impurities rubbed off on the books that passed through it: the message certainly suited the medium. But what a state of affairs! A regime that classified its most advanced philosophy with its most debased pornography was a regime that sapped itself, that dug its own underground, and that encouraged philosophy to degenerate into *libelle.* When philosophy went under, it lost its self-restraint and its commitment to the culture of those on top. When it turned against courtiers, churchmen, and kings, it committed itself to turning the world upside down. In their own language, the *livres philosophiques* called for undermining and overthrowing. The counterculture called for a cultural revolution—and was ready to answer the call of 1789.

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References

1. Marc Bloch, "Critique historique et critique du témoignage," *Annales: économies, sociétés, civilisations,* 5 (1950), 5; reference supplied by Roberto Vivarelli.

2. Daniel Mornet, "Les enseignements des bibliothèques privées (1750-1780)," *Revue d'histoire littéraire de la France,* 17 (1910), 449-492.

3. Although he carefully qualified the conclusions in his article, Mornet made more sweeping statements in his later work. He wrongly implied that his research on the *Social Contract* was valid for the period after 1780: "De ce livre redoutable, c'est à peine si l'on parle avant 1789," Daniel Mornet, "L'influence de J.-J. Rousseau au XVIIIe siècle," *Annales Jean-Jacques Rousseau,* 8 (1912), 44. See also Daniel Mornet, *Rousseau, l'homme et l'oeuvre* (Paris: Boivin, 1950), pp. 102-106, and Daniel Mornet, *Les origines intellectuelles de la Révolution Française,* 5th ed. (Paris: Colin, 1954), p. 229. Robert Derathé accepted Mornet's interpretation: "Les réfutations du *Contrat Social* au XVIIIe siècle," *Annales de la Société Jean-Jacques Rousseau,* 32 (1950-1952), 7-12. And Alfred Cobban extended it: "Rousseau's *Contrat social* had no ascertainable influence before the Revolution and only a very debatable one during its course"; see his "The Enlightenment and the French Revolution," reprinted in *Aspects of the French Revolution* (New York: Braziller, 1968), p. 22. The latest extension is Joan McDonald, *Rousseau and the French Revolution, 1762-1791* (London: Athlone Press, 1965).

4. See the devastating review article by R. A. Leigh, "Jean-Jacques Rousseau," *The Historical Journal,* 12 (1969), 549-565.

5. Although Rousseau's treatise was too abstruse to provoke the sort of controversy that followed *Emile* and his discourses (except in Switzerland), its influence, like all ideological influence, is difficult to measure. If repression is any indication of importance, it should be noted that the French state never formally condemned the *Contrat social* but did not permit it to circulate freely. The revolutionaries found it locked up with other seditious literature in the *pilon* of the Bastille: Bibliothèque de l'Arsenal, Ms. 10305, "le pilon de la Bastille."

6. Escarpit's book was published in the widely-read "Que Sais-Je" series and has already gone through four editions. As an example of its influence, see Louis Trenard, "La sociologie du livre en France (1750-1789)," *Actes du cinquième Congrès national de la Société française de littérature comparée* (Paris, 1965), p. 145.

7. Robert Escarpit, *Sociologie de la littérature,* 4th ed. (Paris: Presses universitaires de France, 1968), p. 46. It should be noted also that the development of writing as a *métier* in the eighteenth century did not correspond to the sociological phenomenon of professionalization. See the article "Professions" by Talcott Parsons in the *International Encyclopedia of the Social Sciences,* XII, 536-547.

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8. David Pottinger, *The French Book Trade in the Ancien Régime, 1500-1791* (Cambridge, Mass.: Harvard University Press, 1958), p. 9.

9. The studies, to be cited henceforth by name of author, are: François Furet, "La 'librairie' du royaume de France au 18e siècle" in *Livre et société dans la France du XVIIIe siècle* (Paris and The Hague, 1965); Jean Ehrard and Jacques Roger, "Deux périodiques français du 18e siècle: 'le Journal des Savants' et 'les Mémoires de Trévoux.' Essai d'une étude quantitative," in the same volume; Daniel Roche, "Un savant et sa bibliothèque au XVIIIe siècle: les livres de Jean-Jacques Dortous de Mairan, secrétaire perpétuel de l'Académie des sciences, membre de l'Académie de Béziers," *Dix-huitième siècle*, 1 (1969), 47-88; François Bluche, *Les magistrats du Parlement de Paris au XVIIIe siècle, 1715-1771* (Paris: Les Belles Lettres, 1960), pp. 291-296, which incorporates the findings of an unpublished study by Régine Petit, *Les bibliothèques des hommes du parlement de Paris au XVIIIe siècle* (1954); and Jean Meyer, *La noblesse bretonne au XVIIIe siècle* (Paris: Imprimerie nationale, 1966), pp. 1156-1177.

10. Furet, p. 19.

11. Ehrard and Roger, p. 56.

12. Mornet, "Les enseignements des bibliothèques privées," p. 473.

13. Quotation cited in Raymond Birn, "Le Journal des savants sous l'Ancien Régime," *Journal des savants* (January-March 1965), p. 28 and in Eugène Hatin, *Histoire politique et littéraire de la presse en France* (Paris, 1859-1861), II, 192.

14. Jean-Louis and Marie Flandrin, "La circulation du livre dans la société du 18e siècle: un sondage à travers quelques sources," *Livre et société*, 2 (Paris, 1970), 52-91. The Flandrins studied three private or at least uncensored literary journals, which discussed philosophic works that could not be mentioned in the pages of quasi-official, heavily-censored periodicals like the *Journal des savants*. But the Flandrins' three journals show the opposite bias from those studied by Ehrard and Roger. They discussed mainly sensational books—books that made news—and thus do not represent the general literary tastes of their readers any more than do the *Journal des savants* and the Jesuit *Mémoires de Trévoux*.

15. Trenard, "La sociologie du livre en France."

16. The main problem in constructing these graphs was to find comparable units and statistics for them in the eight studies under analysis. In order to make comparisons possible, it was necessary to redo some of the mathematics and to reconvert some of the data that appeared in graph form in the two articles published in *Livre et société*. The graphs all refer to the mid-century, although they represent slightly different time spans. The subjects that do not appear on them mostly concern the various "arts" catalogued under the heading "sciences et arts." Because that heading seemed too broad to mean much to the modern reader, it was replaced by the subcategory "sciences." Composed of four subsubcategories—*phy-*

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sique, médecine, histoire naturelle, and *mathématiques*—the "sciences" could be computed in every case except those of Mornet and Bluche-Petit. Mornet gave no statistics on mathematical books, but the omission probably concerned much less than 1 per cent of all his titles and so did not affect the general pattern. Bluche did not differentiate "sciences" from "sciences et arts" at all. Athough the subcategory "sciences" varied widely in other cases (from 10 per cent to 70 per cent of the general category), it seemed reasonable to estimate it at half of Bluche's "sciences et arts" or 7 per cent of the total—a rough approximation indicated by broken lines that the reader may prefer to discount. Mornet's figures covered only "romans" and "grammaires" in the category "belles-lettres," which probably left out a little over half that category, judging from the distribution in Furet's *permissions publiques* and *permissions tacites.* The category probably would have occupied between 10 per cent and 20 per cent of Mornet's total and is indicated as 15 per cent by broken lines. Unlike the others, Mornet did not classify travel literature with history, as was the practice in the eighteenth century. Had he done so, his "history" division would have expanded by another 1.5 per cent. Meyer's "belles-lettres" also is approximate and therefore appears in broken lines.

A graph combining Furet's studies of *permissions publiques* and *permissions tacites* was constructed from computations based on his original data, because it was hoped that an over-all picture of literary output would emerge by combining statistics from those two very different sources. Suggestive as it is, this composite bar graph contradicts all the others. For example, it somewhat resembles the graph based on Mornet's statistics, but Mornet would have the French reading far fewer religious works (6 per cent) in relation to science (3 per cent) and especially history (30 per cent) than would Furet, whose combined graph shows 20 per cent religion, 9 per cent science, and 11 per cent history.

Because all eight studies kept close to the eighteenth-century classification scheme, they do not give much help to the modern reader in search of the Enlightenment. Does he associate Enlightenment with "philosophie," one of the eight subcategories under "sciences et arts"? If so, he must contend with four sibling subsubcategories: *philosophie ancienne, logique, morale,* and *métaphysique.* The last two seem promising, but (except in Roche's statistics, which include two additional subsubcategories) the data do not distinguish them from their two predecessors. The four studies that provide statistics on "philosophie" as a whole suggest it comprised a small, stable portion of eighteenth-century reading: the *permissions publiques* fix it at 3 per cent (1723-1727), 3.7 per cent (1750-1754), and 4.5 per cent (1784-1788); the *permissions tacites* at 6 per cent (1750-1759), 5 per cent (1770-1774), and 6 per cent (1784-1788); the reviews in the *Journal des savants* at 3 per cent (1715-1719), 4 per cent (1750-1754), and 5 per cent (1785-1789); and it made up 7 per cent of Dortous de Mairan's library. Not much evidence for the spread of *lumières.* But then the Enlightenment cannot be identified with any of the eighteenth-century categories or their subdivisions.

It would also be possible to express Pottinger's study of two hundred eighteenth-century authors in a bar graph, because he produced a statisti-

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cal table of their publications, taking Mornet's work as a model. But, as explained above, Pottinger's selection of writers is so arbitrary and his statistics so incomplete and unrepresentative that the graph would not mean very much. Nonetheless, for purposes of comparison, his findings ought to be mentioned (Pottinger, *The French Book Trade*, pp. 30-31): religious works 11 per cent of the total produced by his authors, science 20 per cent, history 20 per cent, law 2 per cent, belles-lettres 10 per cent.

17. This figure is based on the Maggiolo study of literacy as presented by Michel Fleury and Pierre Valmary in "Les progrès de l'instruction élémentaire de Louis XIV à Napoléon III d'après l'enquête de Louis Maggiolo (1877-1879)," *Population* (1957), pp. 71-92, estimating the population at 26 million.

18. Julien Brancolini and Marie-Thérèse Bouyssy, "La vie provinciale du livre à la fin de l'Ancien Régime" and Jean-Louis and Marie Flandrin, "La circulation du livre dans la société du 18e siècle: un sondage à travers quelques sources," both in *Livre et société*, 2 (Paris, 1970). For a detailed discussion of these studies, see Robert Darnton, "In Search of the Enlightenment: Recent Attempts to Create a Social History of Ideas," a review article that will apear in a forthcoming issue of the *Journal of Modern History*.

19. The remainder of this essay is essentially a "work in progress" report based on the first stages of research in the papers of the Société typographique de Neuchâtel in the Bibliothèque Publique de la Ville de Neuchâtel, Neuchâtel, Switzerland, cited henceforth as STN. Other important sources were the papers of Jean-Charles-Pierre Lenoir, *lieutenant-général de police* of Paris from 1774 to 1775 and 1776 to 1785, in the Bibliothèque municipale d'Orléans, Mss. 1421-1423; the Archives de la Chambre syndicale and Collection Anisson-Duperron papers of the Bibliothèque Nationale (especially fonds français, Mss. 21862, 21833, 22046, 22063, 22070, 22075, 22081, 22109, 22116, 22102); the papers of the Bastille and related papers on the book trade in the Bibliothèque de l'Arsenal (especially Mss. 10305, 12446, 12454, 12480, 12481, 12517); and the Ministère des affaires étrangères, Correspondance politique, Angleterre (Mss. 541-549). For information on the underground book route through Kehl and Strasbourg as opposed to Neuchâtel and Pontarlier, the relevant papers in the Archives de la ville de Strasbourg (mainly Mss. AA 2355-2362) were consulted but turned out to be less useful than the others. Research on publishing under the Old Regime by now has made J.-P. Belin, *Le commerce des livres prohibés à Paris de 1750 à 1789* (New York, no date, a Burt Franklin reprint of the original edition, Paris, 1913) somewhat dated. For information about the most important secondary works, see the bibliographies in Nicole Herrmann-Mascard, *La censure des livres à Paris à la fin de l'Ancien Régime, 1750-1789* (Paris: Presses universitaires de France, 1968), and Madeleine Ventre, *L'imprimerie et la librairie en Languedoc au dernier siècle de l'Ancien Régime, 1700-1789* (Paris and The Hague: Mouton, 1958). The present essay was written before the thesis of H.-J. Martin became available, but it relies heavily on his article, "L'édition parisienne au

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XVIIe siècle: quelques aspects économiques," *Annales: économies, sociétés, civilisations,* 7 (July-September 1952), 303-318. Another suggestive article is Léon Cahen, "La librairie parisienne et la diffusion du livre français à la fin du XVIIIe siècle, *Revue de synthèse,* 17 (1939), 159-179.

20. A typical example is the memoire of August 2, 1783, by Périsse Duluc, syndic of the Chambre syndicale of Lyons in the Bibliothèque Nationale, Mss. français 21833, fol. 96.

21. Revol to STN, July 4, 1784, STN Ms. 1205.

22. For example, the Société typographique de Neuchâtel received a letter dated October 30, 1783, from François Michaut, its agent on the Swiss side of the French border, which explained, "Votre partie est assez chatouilleuse à raison de la crainte que les porteurs ont qu'en cas de prise ils ne fussent saisis comme introducteurs d'ouvrages qui attaquent la religion ou qui traitent à dénigrer certaines personnes en place . . . Si vous ne voulez introduire que des livres irrépréhensibles par leur contenu, les porteurs vous demanderont votre garntie pour ces faits là, et vous en trouverez dans nos environs qui vous rendront le quintal à 12 livres de France à Pontarlier ou même une lieue plus loin s'il le fallait. Autre quoi il faut encore donner à boire à chaque porteur avant que de partir. Il faut vous observer, Messieurs, qu'à ce prix là les porteurs font pour le mieux sans vous répondre de la marchandise." Michaut observed with some pride that "effectivement ma position est assez avantageuse pour les entrées clandestines" but warned, "l'on trouve le long de la route et dans les villages des employés ambulants, qui malgré que l'on soit en règle arrêtent et épluchent la charge d'un voiturier." He therefore stressed the need of having an agent to dupe or bribe the employees of the General Farm from the French side of the border: "Je ne connais personne de plus propre à cela que le sieur Faivre," STN Ms. 1183. Faivre did not hesitate to recommend himself. On October 14, 1784, he informed the society, "Samedi prochain vos balles entreront. J'ai tant fait et promis à ces porteurs que je leur donnerait de quoi boire et qu'ils seront contents, ce qui les a ranimés à retourner . . . Je suis au moment de traiter avec un employé des fermes pour nous laisser passer librement la nuit et m'indiquer les chemins où l'on doit passer en sûreté," STN Ms. 1148.

23. STN to J.-P. Brissot, April 29, 1781, STN Ms. 1109.

24. Mme. La Noue to STN, September 8, 1782, STN Ms. 1173. Mme. La Noue was sensitive to complaints that she overcharged and underprotected her customers. On December 9, 1780, she wrote to the STN, "Je vous prie M. vouloir bien aitres tranquille sur le sort de vos objet. Lorsquils sòn entre mais mains je ne neglige rien pour le mettre a labry des evenements. Obligé moy davoir confiance en ma façon de travailler." But on January 13, 1783, she confessed that six of its crates had been seized at her doorstep: "Le voituriè etoit suivi au point quil cest trouvè icy a la de charge dudit voituriè 3 personne de la prevottè qui ce sont emparrè des ditte 6 balles et de lettres de voiture que le voituriè na put leurs reffuzè par les menaces quils luy on fait et amoy bien des question pandant quinze jour pour

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declarrè les personnes a qui apartenoit les dittes balles et dou elle venoit aquoy je me suis reffuzè," *ibid.*

25. Paul de Pourtalès to STN, June 23, 1784, STN Ms. 1199.

26. See the Desauges dossier in the Bibliothèque de l'Arsenal, Ms. 12446. On April 4, 1775, Desauges père wrote dyspeptically from the Bastille to his son, who had just been released, "il faut prendre son mal en patience. Je t'avouerai franchement que je m'ennuie à la mort." The Desauges dossier in Neuchâtel, Ms. 1141, shows the sharp practices of underground book-dealers at their most cutthroat.

27. Mme. J. E. Bertrand to STN, October 7, 1785, STN Ms. 1121.

28. J.-F. Bornand to STN, August 10, 1785, STN Ms. 1124. Poinçot occasionally smuggled books from Versailles to Paris for Desauges at twelve livres the quintal, which apparently was cheap compared with the charges of Mme. La Noue: three livres per "gros objet," which her nephew delivered to appointed hiding places on the outskirts of Paris (see Desauges to STN, November 24, 1783, Ms. 1141 and Mme. La Noue to STN, June 22, 1781, Ms. 1173).

29. Among his tasks, Bornand had to try to cope with the "verbiages" of Mme. La Noue (Bornand to STN, February 19, 1785, Ms. 1124), the ruses of Poinçot and Desauges, and the impecuniousness of authors: "C'est une triste ressource que les auteurs pour l'argent" (Bornand to STN, March 9, 1785, *ibid.*).

30. Giles Barber, "French Royal Decrees Concerning the Book Trade 1700-1789," *Australian Journal of French Studies,* 3 (1966), 312.

31. A. J. L. Jourdan, O. O. Decrusy, and F. A. Isambert, eds., *Recueil général des anciennes lois françaises* (Paris, 1822-1833), XXI, 230.

32. *Ibid.,* p. 218.

33. *Ibid.,* p. 217.

34. George V. Taylor, "Noncapitalist Wealth and the Origins of the French Revolution," *American Historical Review,* 72 (1967), 469-496.

35. P. J. Blondel, *Mémoire sur les vexations qu'exercent les libraires et imprimeurs de Paris,* ed. Lucien Faucou (Paris, 1879); see especially pp. 18-25 and 45.

36. Quotations from *Recueil général des anciennes lois françaises,* XXV, 109, 119, and 110 respectively.

37. *Ibid.,* p. 109.

38. Because of the complicated problems of dating Diderot's *Lettre,* relating it to earlier documents that influenced Diderot's argument, and establishing a correct version of the text, it is important to read the *Lettre* in the critical edition by Jacques Proust (Paris: Colin, 1962). But even the old edition in Diderot's *Oeuvres complètes,* ed. J. Assézat and Maurice Tourneux

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(Paris, 1876), XVIII, 6, included a note by someone in the *Direction de la Librairie* (d'Hémery?) which observed that Diderot wrote the *Lettre* "d'apres le conseil des libraires et sur des matériaux que M. Le Breton . . . lui a fournis, et dont les principes sont absolument contraires à la bonne administration des privilèges." Although the *Lettre* contains some heartfelt statements about liberty and the tribulations of authors, its logic is twisted to favor publishers and it reproduces the old arguments advanced by the guild. It is therefore difficult to accept Brunel's claim that Diderot did not write the *Lettre* either as an ally or as a paid propagandist of Le Breton and the other privileged publishers: Lucien Brunel, "Observations critiques et littéraires sur un opuscule de Diderot," *Revue d'histoire littéraire de la France,* 10 (1903), 1-24.

39. The code of 1777 weakened some of the Parisian guild's power by giving authors the right to sell their own works and by providing for two public book sales in Paris every year. It favored provincial publishers by permitting them to print the increasing number of books that it caused to fall into the public domain—an acknowledgment of the fact that they had engaged in illegal activities for lack of "un moyen légitime d'employer leurs presses," *Recueil général des anciennes lois françaises,* XXV, 109. The edicts of 1777 thus attempted to "faire cesser la rivalité qui divise la librairie de Paris et celle des provinces, de la faire tourner au profit de cette branche importante du commerce, et de former de tous les libraires une même famille qui n'aura plus qu'un même intérêt," *ibid.,* pp. 119-120. But this rivalry went too deep to be settled by such small concessions to the provincial dealers, who continued to protest against exploitation by the Parisians throughout the 1780's. The 1777 code also extended and strengthened the guild system in the provinces, because "S.M. a reconnu qu'il serait dangereux de laisser subsister les imprimeries isolées dans un état d'indépendance qui y facilite les abus," *ibid.,* p. 112. So the reorganization of the guilds did not substantially weaken them or impair their policing function.

40. D.-J. Garat, *Mémoires historiques sur la vie de M. Suard, sur ses écrits et sur le XVIIIe siècle* (Paris, 1820), I, 274.

41. Bibliothèque Nationale, Mss. français 21833, foll. 87-88. This account of French tax and tariff legislation is derived from several documents in Ms. 21833, particularly foll. 89-91 and 129-140.

42. The tariff legislation was a constant theme in the commercial correspondence of the Société typographique de Neuchâtel for the first half of the 1770's. The society even sent one of its partners on a business trip through eastern France to sell books, to find new ways of making fraudulent shipments, and to learn as much as possible about tariff policy. According to the instructions in his travel-log, he was to seek out "J. M. Bruysset, homme froid et habile: 1° S'entretenir avec lui de la librairie française en général, savoir de lui si en effet l'impôt sera levé ou diminué," STN, Ms. 1058, "Carnet de voyage, 1773, J. E. Bertrand." The Bruysset house was one of the most effective lobbyists against the tariff, judging from the memoranda in the Bibliothèque Nationale, Mss. français 21833, especially foll. 87-88 and 129-140. The tariff damaged the illegal trade, because pirated works

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were usually shipped through legal channels, at least at the border, under false *acquits à caution* and therefore paid duty.

43. Bibliothèque Nationale, Mss. français, 21833, foll. 87-88. This memoire reads as though it were the work of Panckoucke. One sou per gathering was the normal printing charge of the Société typographique de Neuchâtel, whose flourishing business in the late 1770's seems to have resulted from the combination of France's favorable tariff policy and the cheap conditions of printing in Switzerland.

44. *Ibid.*, foll. 111-115. The dealer showed, by a very detailed argument, that a six-hundred-pound crate would cost him 61 livres, 15 sous in extra charges, would cause enormous delays and damage through mishandling, and would make it impossible for him to collect insurance for damaged shipments.

45. *Ibid.*, fol. 70.

46. *Ibid.*, fol. 107: "Les libraires éloignés de Paris, et ceux de Lyon en particulier, ont sur le champ contremandé les envois qu'on devait leur faire, fait rétrograder les ballots qui étaient en route, annulé leurs marchés, et renoncé aux entreprises d'impression pour lesquelles ils se voient maintenant sans débouchés. Enfin, il n'existe déjà plus de correspondance active entre les libraires français et les libraires étrangers."

47. J. F. Bornand to STN, April 12, 1784, STN Ms. 1124.

48. J. F. Bornand to STN, April 9, 1784, *ibid.*

49. J. F. Bornand to STN, February 19, 1785, *ibid.*

50. STN to Garrigan, a bookdealer in Avignon, August 23, 1785: "Nous partageons sans doute bien sincèrement le regret sur l'interruption de notre correspondance que vous voulez bien nous témoigner par l'honneur de votre lettre du 10 de ce mois, mais vous n'ignorez pas que la cause fatale ne peut en être attribué qu'à la rigueur toujours subsistante des ordonnances concernant l'introduction de la librairie étrangère dans le royaume. Les choses sont encore sur un tel pied à cet égard, que nous ne pouvons faire entrer une balle *Libri* par le bureau de notre frontière qu'en prenant un acquit à caution pour Paris, où les vôtres seraient obligés d'aller en faisant ainsi un détour immense et subissant l'examen de la Chambre syndicale Parisienne, ce qui est absolument impraticable," STN Ms. 1110.

51. STN to Mme. J. E. Bertrand, early October 1785, STN Ms. 1110.

52. The archives in Strasbourg, an important center of the clandestine trade, complement those in Neuchâtel in that they show a determined effort by the government to stop traffic in prohibited works. Strasbourg's *préteur royal* received frequent reports from local officials about seizures of illegal shipments from the publishers across the Rhine; and he also received strict orders from his own superior, the *garde des sceaux* (letter of April 26, 1786, in Archives de la Ville de Strasbourg, Ms. AA2356): "La librairie proscrite par nos lois vous environne de toute part; et elle pénétréra par les moyens

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que vous ne lui aurez pas interdits, si vous ne les lui interdisez pas tous . . . Je vous exhorte donc fortement, vous et le magistrat de votre ville, à prendre les mesures convenables." Despite this rigor, printers in Kehl seem to have got a great many books—political pamphlets and *libelles* as well as Beaumarchais' Voltaire—through the traps laid for them in Strasbourg. The town's semiautonomy, guaranteed by the capitulations of 1681, may have made it relatively easy to penetrate.

53. See Furet, p. 8, and Robert Estivals, *La statistique bibliographique de la France sous la monarchie au XVIIIe siècle* (Paris and The Hague: Imprimerie nationale, 1965), p. 296.

54. Bibliothèque Nationale, Ms. français 21833, fol. 107.

55. *Ibid.*, fol. 108; see also foll. 99-104.

56. Vergennes to d'Adhémar, May 12, 1783, Ministère des affaires étrangères, Correspondance politique, Angleterre, Ms. 542. The details of "cette machination d'intrigues, de cupidité, et de fourberie," as Vergennes called it (Vergennes to Lenoir, May 24, 1783, *ibid.*)—and which I plan to recount in a later work—can be found in the series 541-549.

57. Bibliothèque de l'Arsenal, Ms. 10305. The inventory also included *Le gazetier cuirassé, L'espion dévalisé, Vie privée de Louis XV, Le diable dans un bénitier,* and other classics of the London School of *libellistes.* It specified that they had been shipped to some of the customers of the Société typographique de Neuchâtel, notably Poinçot, Blaizot, and Mme. La Noue. Poinçot himself drew up the inventory.

58. For the conventional view that the government's policy was severe in theory and permissive in practice, see J.-P. Belin, *Le commerce des livres prohibés à Paris de 1750 à 1789* (New York, no date, a Burt Franklin reprint of the original edition, Paris, 1913) and the restatement of Belin's interpretation in Nicole Herrmann-Mascard, *La censure des livres à Paris à la fin de l'ancien régime, 1750-1789* (Paris: Presses universitaires de France, 1968). Both books dismiss the June 12, 1783, orders in two sentences—the same sentences, curiously, almost word for word (Belin, p. 45; Herrmann-Mascard, p. 102).

59. It also might serve as a corrective to the Marxist tendency to treat the Enlightenment as bourgeois ideology. One version of this tendency argues that ideas such as social contract, individualism, liberty, and equality before the law derived from capitalist methods of exchange, which involve contractual obligations between legally free and equal individuals: Lucien Goldmann, "La pensée des 'Lumières,'" *Annales: économies, sociétés, civilisations,* 22 (1967), 752-770. Considering the multitude of writers who expressed such ideas before the development of capitalism, this argument seems less convincing than its opposite, which relates the Enlightenment to a tradition of aristocratic liberalism: Denis Richet, "Autour des origines idéologiques lointaines de la Révolution française: élites et despotisme," *ibid.*, 24 (1969), 1-23.

60. STN, Ms. 1108.

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61. *Ibid.* In contrast, the manuscript catalogue offered the following under the letter "B": "*La belle allemande, ou les galanteries de Thérèse,* 1774; *Bijoux indiscrets* par Diderot, 8° figures; *Le bonheur,* poème par Helvétius; *Le bon sens, ou idées naturelles, opposées aux idés surnaturelles.*"

62. Reprinted in A. Van Bever, *Contes et conteurs gaillards au XVIIIe siècle* (Paris: Daragon, 1906), pp. 280-281. In notes that he assembled for his memoirs, the former lieutenant general of police J.-C.-P. Lenoir associated this work with a widespread outbreak of libeling in the 1780's (Bibliothèque municipale d'Orléans, Ms. 1423): "Les moeurs du successeur de Louis Quinze étant inattaquables, le nouveau roi fut inaccessible de ce côté à la calomnie pendant les premières années de son règne, mais on commença en 1778 à le diffamer du côté de sa faiblesse, et les premières calomnies qui furent ourdies contre sa personne ne préludèrent que de trés peu de mois ceux de la méchanceté contre la reine. M. de Maurepas, qui jusques là avait été fort insouciant touchant des épigrammes et des chansons faites contre lui, M. de Maurepas, qui s'amusait de tous les libelles, de toutes les anecdotes privées et scandaleuses qu'on fabriquait et imprimait avec impunité, eut avis que des écrivains avaient fait entre eux une sorte de spéculation, qu'ils avaient lié une correspondance au moyen de laquelle les uns envoyaient de Paris les histoires courantes et fournissaient des titres et des matériaux à ceux qui les composaient et faisaient imprimer à La Haye et à Londres, d'où ils les faisaient entrer en France en petite quantité par des voyageurs étrangers. Un secrétaire d'ambassade d'Angleterre lui annonça qu'on devait incessament y introduire en France un libelle abominable intitulé *Les amours de Charlot et d'Antoinette.*"

63. *Le portefeuille d'un talon rouge contenant des anecdotes galantes et secrètes de la cour de France,* reprinted under the title *Le coffret du bibliophile* (Paris, no date), p. 22. Lenoir's manuscripts confirm this account (Bibliothèque municipale d'Orléans, Ms. 1422): "Il n'est plus douteux maintenant que c'étaient MM. de Montesquiou, de Créqui, de Champcents, et d'autres courtisans, qui de concert avec Beaumarchais, Chamfort, et autres écrivains vivants encore avaient composé des libelles contre la cour, contre les ministres, et même contre ceux des ministres qui les employaient. Il est plus que probable que Beaumarchais avait composé, porté à Londres, où il a été imprimé, un libelle avec figures gravées intitulé *Les amours de Charlot et d'Antoinette.*"

64. *Ibid.*

65. *Ibid.* Lenoir's remarks might seem to contradict the above interpretation about a crackdown on underground publishing, but they refer primarily to the circulation of *libelles* inside Paris, not to the traffic from outside France to the capital. There seems to have been a considerable domestic production of *libelles,* which survived the police's attempts to impound them because of influential "protection" and the immunities of "lieux privilégiés" like the Palais-Royal, where the police could not penetrate. See *ibid.,* Ms. 1421.

66. See Robert Darnton, "The Grub Street Style of Revolution: J.-P. Brissot, Police Spy," *Journal of Modern History,* 40 (1968), 320-321.

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67. Lenoir later tried to investigate the rumor and the riot but without success: Bibliothèque municipale d'Orleans, Ms. 1422.

68. Lenoir developed his observations most fully in an essay entitled "De l'administration de l'ancienne police concernant les libelles, les mauvaises satires et chansons, leurs auteurs coupables, délinquants, complices ou adhérents," *ibid.*

69. [Charles Théveneau de Morande], *Le gazetier cuirassé: ou anecdotes scandaleuses de la cour de France* ("imprimé à cent lieues de la Bastille à l'enseigne de la liberté," 1771), p. 92: "La nation française est si mal constituée aujourd'hui, que les gens robustes sont sans prix: On assure qu'un laquais qui débute à Paris est payé aussi cher par les femmes qui s'en servent qu'un cheval de race en Angleterre. Si ce système prend faveur, une génération ou deux suffiront pour rétablir les tempéraments." In *Le libertin de qualité,* reprinted in *L'oeuvre du Comte de Mirabeau,* ed. Guillaume Apollinaire (Paris, 1910), Mirabeau described aristocratic immorality in great detail. After recounting a depraved duchess' abandonment of her lover, he remarked (p. 232), "Elle l'a remplacé par un prince, et réellement, quant au moral, ils se convenaient; pour le physique, elle eut ses laquais: c'est le pain quotidien d'une duchesse."

70. [Charles Théveneau de Morande], *La gazette noire par un homme qui n'est pas blanc: ou oeuvres posthumes du gazetier cuirassé* ("imprimé à cent lieues de la Bastille, à trois cent lieues des Présides, à cinq cent lieues des Cordons, à mille lieues de la Sibérie," 1784), pp. 194-195.

71. See Richard Cobb, "Quelques aspects de la mentalité révolutionnaire," *Revue d'histoire moderne et contemporaine,* 6 (1959), 81-120, and "The Revolutionary Mentality in France," *History,* 42 (1957), 181-196.

72. *La gazette noire,* p. 7. For a similar example of such rumors about the promiscuous use of police by "gens en place" see M. de Lescure, ed., *Correspondance secrète inédite sur Louis XVI, Marie-Antoinette, la Cour et la ville de 1777 à 1792* (Paris: H. Plon, 1866), II, 157-158.

73. The subheadline of the account of the queen's guillotining in *La Père Duchesne.*

74. Pierre Goubert, *L'Ancien Régime* (Paris: Colin, 1969), I, 152.

75. Bibliothèque municipale d'Orléans, Ms. 1423.

76. Quoted in Frantz Funck-Brentano and Paul d'Estrée, *Les nouvellistes* (Paris: Hachette, 1905), p. 304.

77. Goubert, *L'Ancien Régime,* p. 152. The connection between privilege and monopoly is brought out clearly in the first definition of "privilège" given in the *Dictionnaire de l'Académie française* (Paris, 1778): "Faculté accordé à un particulier ou à une communauté de faire quelque chose ou de jouir de quelque avantage à l'exclusion des autres."

78. See Charly Guyot, *De Rousseau à Mirabeau: pèlerins de Môtiers et prophètes de 89* (Neuchâtel and Paris: Victor Attinger, 1936), chap. 4.

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79. Although the increased severity in the policing of the book trade cut down on its business in France, the Société typographique de Neuchâtel still did its best to supply works like the following, which it entered in its "Livre de commission" (STN Ms. 1021, foll. 173-175) after receiving an order from Bruzard de Mauvelain, a clandestine dealer in Troyes, dated June 16, 1784: "6 *Les petits soupers de l'Hôtel de Bouillon;* 6 *Le diable dans un bénitier;* 6 *L'espion dévalisé;* 1 *Correspondance de Maupeou;* 1 *Recueil de remontrances au Roi Louis XV;* 2 *Mémoirs de Madame de Pompadour;* 2 *Vie privée de Louis XV;* 12 *Fastes de Louis XV;* 6 *Histoire philosophique* 8°, 10 vol; 6 *Erotika biblion* 8°; 1 *La Mettrie;* 1 *Boulanger complet, antiquité, Christianisme, et despotisme;* 1 *Helvétius complet;* 6 *Lettres de Julie à Calasie, ou tableau du libertinage à Paris;* 1 *la dernière livraison de Jean-Jacques* 12°; 6 *Chronique scandaleuse;* 6 *Les petits soupers du comte de Vergennes;* 6 *Le passe-temps, d'Antoinette.*"

80. See Paul Robiquet, *Théveneau de Morande: étude sur le XVIIIe siècle* (Paris: A. Quantin, 1882).

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